BENJAMIN GARVER LAMME

AN

AUTOBIOGRAPHY

Benj. G. Lamme

"In my thirty-five years of work with the Westinghouse Company I have seen many young men grow from pupils to assistants and associates. This has been one of my greatest pleasures. I have aimed to instill in them fundamental ideas of engineering honesty and honor, square dealing and fair fighting —that there should be pride in accomplishment because true engineering means much more than merely making a living; it means advancement of the art for the benefit of mankind."

BENJAMIN G. LAMME.

BENJAMIN GARVER LAMME

ELECTRICAL ENGINEER

AN

AUTOBIOGRAPHY

He "viewed hopefully the hitherto unattainable"

G. P. PUTNAM'S SONS

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EDITORIAL NOTE

MR. LAMME, in writing an introduction for the book of a friend, used the striking phrase "Unfortunately, it is only in rare cases that the *doer* is also the *teller*." His own story here presented is one of these notable exceptions, and his doing is much graced by his telling.

As was his custom in writing, after this manuscript was nearly complete as a record of his first thirty years of engineering work, he began a revision to refine the material, at the same time deciding to include more details of his life and personality than were contemplated in the first plan. This revision he never completed. In editing these two partial manuscripts to form a unified and consecutive history, it was felt essential to include all possible material from both his original manuscript and his revision. The reason for this was that in commenting at different times on the same event or the same apparatus, he used the most interesting versatility in developing different aspects of the same material.

So, in justice to his literary style, which was always clear and concise, it should be understood that if the reader detects certain passages which seem reminiscent of something already discussed, it is not the fault of the author, nor editorial carelessness, but an effort to give to his readers everything possible from his own memoranda of a long and busy life covering the richest period of pioneer

development in the art of designing dynamo electric machines.

In the same manner, an effort has been made to preserve his characteristic virility of expression rather than to change forms to something no more nearly correct but perhaps more conventional. In short, he used language as he used all his tools—as a means to an end and not as the end itself—and it was seldom possible to modify his phrases without detracting from the effect he wished to produce. His manner of expression was part of his personality and shares in his charm.

MANSFIELD DUDLEY.

OAKMONT, PA.,
November, 1926.

A sixteen-year-old school boy on a farm in Western Ohio puzzling over the description of an Edison dynamo in "Harper's Weekly" of January third, 1880, was fascinated and inspired by a determination to make electrical machines.

Before another sixteen years had passed, he had designed the great generators for Niagara which were without precedent. They were marvels of engineering achievement and ushered in a new era in electrical power.

THE boy who did this was B. G. Lamme, and in the following pages he tells in simple language the unique story of his unique life. His fame rests upon his work as engineer and designer, but to those who knew him his personal and intellectual qualities also won respect and admiration, and many who read his story are likely to find the revelation of a rare personality of far greater interest than the record of technical achievement. The natural aptitudes, the kind of education and training, the methods of thinking and working, which contributed to the making of a great engineer, a man who could do things and one who had his hobbies as well as his profession—these are the features of real interest in this autobiography. When Lamme tells of his technical work, the simple account of the steps he takes and of his persistence in overcoming recurring difficulties adds a sort of romance so that even his published Technical Papers have been found significant and inspiring when read by one who does not understand

the machines he describes. This self-written biography is the story of a unique life—happily one which was rich both in itself and in its enduring contributions.

To appreciate the kind and quality of his work, the reader must have in mind the conditions in the electrical industry when he entered it and the remarkable developments which followed rapidly. To these he contributed an outstanding part.

During the 80's, while he was in school and college, electric lighting and railways had their commercial beginning. The particular problems of those years were to design and make dynamos and motors which were larger and more dependable and to transmit electric current to greater distances and make it more generally useful.

In 1890 electric service was practically limited to local lighting and the operating of small electric railways. Few dynamos exceeded one or two hundred horse power, and different types were necessary to furnish current for arc lamps, for incandescent lamps (some direct current, some alternating current) and for street cars. Dynamos and motors were of many types and forms, the product of many inventors, mainly by cut-and-try methods.

There was a long and bitter controversy—scientific and commercial—as to whether direct current or alternating current should prevail in future developments. Mr. Westinghouse had introduced the alternating current in this country and was fighting its battles. Single phase lighting plants were rapidly increasing in number and he had recently acquired the Tesla polyphase patents. The first and great need was for generators and motors for translating into practice the schemes of the inventor. Cut-and-try methods were unavailing.

It was at this critical period that Lamme began his work. In the Westinghouse testing room he solved successfully minor and then larger difficulties; he proposed new methods and new designs. He did not guess, he calculated; he did not rely upon abstract formula, but visualized his problem; he analyzed the conditions; he proceeded by direct and simple steps from fundamental principles to concrete results.

Witness his consummate skill applied to the railway motor—his first major achievement. There were many kinds of motors of many types; he did not imitate them, but with a single master stroke he produced a new type, one whose outstanding half dozen features still persist as the universal standard after thirty-five years of service. This was not the outcome of a brilliant flight of the imagination of a genius, but it was the result of a clear understanding of underlying principles, a careful analysis of the problem, a constructive imagination for devising new methods and the painstaking calculation which transformed a vision into a reality.

During the '90's electric power made its great advance. It was the period of transition from small experimental beginnings to the scientific methods which have made modern electric power possible.

During that eventful decade when the many types of empirical machines and the many electrical systems fashioned by individual inventors evolved into scientific types and a single universal system—the polyphase alternating current system Lamme was in his prime and at the forefront. Scores of inventors had been making dynamos for a decade or more, and scores were competing in design during the '90's, but he was leader; to him are due more

designs which have persisted than to any other. He blazed the path of scientific rational design.

During the three score years of Lamme's life, the curves of progress turn sharply upward; Industry, Production, Wealth increased five or ten fold more in those sixty years than in the preceding sixty centuries and the civilization of the world entered a new epoch. The great event of the nineteenth century was the use of mechanical power; the steam engine replacing the meager power of men and of animals by an unlimited power for operating railways and industry produced an industrial revolution which changed the life of the world—industrial, commercial and social. Then came electricity; the development of the '90's made possible the great transition to electrical methods rendering the power of engine and water wheel marvelously more effective, through transmission and conversion into light and heat, as well as enormously extending the use of mechanical power by means of motor drive. Electricity assumed its great rôle as universal servant of mankind, to do the work of the world.

And Lamme was a foremost leader in this electrical development. He won his place, not by some new discovery in the field of research physics, not through some lucky strike in a new field nor in a spectacular achievement to be heralded in startling headlines. His success was won primarily through engineering analysis and by hard work; he "viewed hopefully the hitherto unattainable" and by patient persistence he proceeded step by step until he achieved his goal—which again and again was a new starting point for new effort. He was a master builder of the machinery and the methods and the system which have made the twentieth century the era of electric power.

When Lamme tells of his early boyhood aptitudes and traits, tracing their development through school and college years, to characteristics in his productive years, he is studying himself as he studied others. He is not writing a general history of the development of electrical apparatus from the small beginnings of 1889 through the succeeding eventful decades but he is telling his own individual story.

He writes in the first person, but he is thinking of himself in the third person. If he seems to commend himself or his achievements, he is doing just what he would do if he were writing about another person. He guards his achievements with jealous pride, but he meticulously would scorn credit which was due to another worker. Designers were then secretive and avenues of intercourse were limited. He himself remarked that he did not feel competent to pass with discriminating judgment on the work of others and he would not even risk saying much about his immediate co-workers lest through oversight or omission he should seem to be unfair.

Just what he did and how he did it and what sort of a man he was who did these things—all of this is told in his own story, sometimes in the lines, sometimes between the lines. It is replete with human interest. What he did was often of less interest than his way of doing it. True he was an electrical engineer, an analyst and designer, and much of his work involved specialized studies and particular designs, but he had a clear perspective and a good sense of proportion, and he had many and varied interests and sympathies. Simple and homelike, quiet and retiring in manner and tastes, he had a geniality and comradeship and an enthusiasm for nontechnical

things which was appreciated by those who were close to him, and even they will learn much about him that is new to them as they read what he has written.

His interest in young men was significant; he aided and guided and trained; he sought to leave a group of young men familiar with his methods to carry on his work. And it is certain that he cherished the hope that this intimate record of his own life might be helpful to other young men.

At the Editor's request, Mr. Charles F. Scott, a life long personal friend and associate has contributed this Foreword to give those who did not know Mr. Lamme a general perspective of his life work.

CHRONOLOGICAL RECORD

BENJAMIN GARVER LAMME
son of
JAMES GIVEN LAMME and SARAH GARVER LAMME
Born January 12, 1864, near Springfield, Ohio
Graduated from Ohio State University in 1888 with the degree of Mechanical Engineer
Entered the Westinghouse Electric and Manufacturing Company, May 1, 1889
Assistant Chief Engineer, 1900–1903
Chief Engineer, 1903–1924
Naval Consulting Board of the United States, 1915–1919
Edison Medal of the American Institute of Electrical Engineers, 1919
Sullivant Medal of Ohio State University, 1923
Died July 8, 1924, at Pittsburgh, Pennsylvania

CONTENTS

APPENDIX

CONTENTS

ILLUSTRATIONS

ILLUSTRATIONS

BENJAMIN GARVER LAMME

AN AUTOBIOGRAPHY

CHAPTER I

FROM BIRTH THROUGH COLLEGE

I WAS born near Springfield, Ohio, and received my early schooling, including high school training, at the local schools near my home.

My earliest recollections go back to the time when I was about four years of age, and only a few incidents of that period stand out. I remember going, at this age, out into the middle of a large plowed field to look for certain curious stones. On my father's farm were many Indian relics, such as stone axes, pestles and hammers; and probably in my earliest years I had been told what they were and so had acquired an interest in them. Presumably on account of this early interest, when very young, I took to gathering such things; and, in the following years, formed quite a large collection of them. This did not indicate anything to me until many years after, as I shall explain later.

From my earliest remembrance, I liked to work with tools, and had much pleasure in my few toys. When I was about five years old, my brother was given a set of wooden building blocks for Christmas. These blocks appealed to me more strongly than anything I had received, so much so, that on New Year's day my father gave me a similar set, although of much cheaper construction. However, this cheapness made no difference to me so long as I had the blocks to work with. I even considered,

during the week's interval, the question of making them, using the materials at my command; and evidently I was as well satisfied with the workmanship of my own toys as with that of those which could be purchased, no matter how poor this workmanship was.

As far back as I can remember, I always liked to tinker around machinery; but in farm equipment there was not much machinery that a child could work with. Nevertheless, I took whatever opportunities I could find. It developed later that it was not the mere working with machinery that was the real pleasure to me, but the finding out how things worked and why they were made in certain ways.

Much "tommy-rot" had been written regarding some of the things which it is claimed I had done, as a child, with machinery, clocks, and various things. Most of this is utterly absurd. I did not make a business of tearing down and reassembling neighbors' clocks and other pieces of domestic apparatus, for they all knew too much to permit me to undertake such things, even if I had wanted to do so. My principal interest in clocks was to know how and why the various combinations were made, how the escapements worked, and how the striking parts were arranged. It was necessary for me to take one or two clocks apart (discarded ones at that) until I found the general scheme of operation; and after that, clocks did not interest me so much. I remember when I was a small boy, a journeyman clock-repair man came to the house and overhauled our large clock. He took everything apart and laid the parts on a large table. I sat across the table from him, watching every operation, and asking him innumerable questions as to why things

The Old Homestead Near Springfield, Ohio.

were made in certain ways and what certain combinations were for. Undoubtedly, I had him puzzled a good many times; for, toward evening, I observed him looking at me covertly many times, and I wondered then why he was watching me.

In my early boyhood, I was so situated that there were available very few tools or machines with which I could work, except the usual saw, hammer, chisel and files. Consequently, I early acquired the same habit regarding tools that I have spoken of in regard to toys: when I needed any special tool, I would devise and make it, usually of the crudest sort, but adequate for accomplishing the desired result. The conditions necessitating my doing this, which at the time appeared to me as most unfortunate, I now look upon as representing a very useful part of my early training.

Like most children, I preferred things which would rotate, but I had a special liking for things that rotated at exceptionally high speeds. I remember once, in my early boyhood days, when I devised a scheme for rotating an ordinary thread spool at an excessively high speed, I immediately set to work to put it into effect. For operating appliances, I had at hand, a grindstone, a wheel barrow and the spool. I turned the wheel barrow upside down, in line with the grindstone; I ran a cord from the grindstone to the wooden axle of the wheel barrow; then I belted a second string from the barrow's wheel rim to the spool, which I mounted to rotate on a wooden shaft carried between two wooden uprights driven into the ground. I figured that at 150 revolutions per minute of the grindstone, I would get about 30,000 revolutions of the spool, not allowing for slippage, of which

I knew nothing at that time. Also, knowing little about lubrication, I did not grease the shaft of the spool. In consequence, while the grindstone was speeded up slowly, the spool got into rapid motion, and at about one-half to two-thirds speed, the spool shaft caught fire, and I had to stop. I remedied this trouble by putting in a piece of heavy wire, instead of wood, for the spool shaft. After several attempts, I again got the set in motion and the spool reached such a frightful speed that it gave a very high pitched screech. The spool then suddenly disappeared completely, and I was unable to find any traces of it. According to my crude figures made at the time, the spool had reached a speed of nearly 30,000 revolutions per minute. Knowing but little about the strength of materials or the effects of high peripheral speeds, I was not at that time aware that the spool must have burst, the bursting being the cause of its sudden disappearance. However, I was quite highly pleased that I had succeeded in running something which to me, represented an entirely unheard of number of revolutions.

I cite the above simply as an illustration of some of the "stunts" that I was attempting as a youngster, on account of which I was always being condemned for "doing such foolish things." I do not know but that, possibly, my appetite for high speed turbo-generators is a reflection of this early desire for high speeds. I did other things in those days, some of which were more foolish, and possibly even more dangerous than the feat just described. For instance, I exploded with a hammer a number of revolver cartridges to see what would happen. I also attempted to cut the "cap" off an old muzzle-loading gun by means of a cold chisel, getting one of

my fingers badly punctured by a piece of the copper cap.

Nevertheless, in my earliest experiences, as a boy, I discovered many things in the "horse sense" line, which have been useful to me ever since. As an illustration, I recall that I fixed up a very simple scheme for melting some lead and tin scrap and putting it into ingots. A few years ago, the Westinghouse Company had an accident in one of its metal furnaces, with the resultant escape of zinc to the extent of some 14,000 or 15,000 lbs., which collected in the form of one huge chunk. After various attempts on the part of the metal workers to handle this chunk, the accident came to my attention, and I mentioned, in an offhand way, the manner in which I had handled tin and lead in my boyish experiments. Immediately, this scheme was used to handle the big mass of zinc; the result proved quite successful in putting it into the form of ingots.[1] I tell this to illustrate that

[1] As I recall the incident referred to by Mr. Lamme, a hot galvanizing kettle containing about 15,000 pounds of molten zinc had developed a leak during the night and the entire contents had run into the ash pit and solidified into one large lump. A new kettle had to be installed and the furnace replaced. The large lump of zinc was lifted from its original position by means of a crane and set to one side until its disposition had been decided upon. By sledging small portions of the thin edge of the mass, it could be broken off, but no headway could be made in this manner. The use of a large tup or drop weight was also ineffective. Attempts to cut up the mass of metal by means of a torch were also unsatisfactory, due to the heat conductivity of the metal. Mr. Lamme's suggestion was to make an open-air furnace, consisting of several large pieces of sheet iron, slightly bent up at the edges and sloped from the back to the front. These sheets were supported upon bricks, so that they were some little distance from the ground. The mass of zinc was placed upon the sheets and a slow fire built beneath. This gradually melted the zinc, just as a lump of ice is melted and the metal, as it trickled down, was received into ingot molds and later utilized in the new kettle.—J. L. Jones, Material & Process Engineering Department Westinghouse Electric and Manufacturing Company.

all my early notions were not really as absurd as they looked at that time.

As a small boy I had great interest in making things, but in later years I concluded that this was only indicative of a general interest for certain things. It was an interest for design, in the general sense, that in my case, was apparently back of all of this, for this taste appeared in various other ways at different times. In other words, in attempting to make things, it was not the mere operation of making them that was the greatest pleasure; it was the general scheming out that apparently counted most, and the attempt to make them being largely to prove whether my ideas were right or wrong.

In fact, I found, in later years, that practically all my early childhood tastes had been indicative of something; and many of these tastes, in some form or other, had grown stronger with the years. However, there was one particular aptitude which I had in early childhood which has possibly proved to be of more importance than all others, namely, my liking for certain phases of elementary mathematics. When I was quite young it was my good fortune (at one time I thought it was a misfortune) to attend an old-fashioned country school. The kind of teaching that they did in those schools seems to have been just what I needed in many ways, for the teacher permitted the pupils to work things out pretty much in their own way, being usually so crowded for time that he was anxious to develop short-cuts and quick ways of doing things.

I had an early taste for arithmetic. Mental arithmetic was particularly interesting to me and was of superlative benefit. One of the things in which I was especially in-

At 4 years.

At 15 years.

Early Portraits.

terested in my early study of arithmetic was the relations of numbers. Many of these I discovered myself, but did not know at the time just why they were so. I could work for hours at this subject. I developed curious and quick methods for carrying on mental calculations and I practiced some of these outside as well as within school. When I was a small boy I believed my memory was quite faulty, chiefly because I could not repeat by rote what the other students could. They could get up and recite just exactly what the book said; I could not, but had to explain things in my own language, often in a very awkward manner. Being very much ashamed of this condition, I attempted to correct it one winter by working very hard to commit things to memory. After a whole winter's work, I found that I still could not commit long passages to memory and thought my winter's work had been pretty much a failure. But later, I discovered that my memory for numbers and numerical relationships had very much improved, and that mental arithmetic apparently had been made easier by this method. At the time, I was not at all pleased with my efforts in memory training, but had reason, years afterwards, to feel that it really had been a very great success.

I had gone through what was called "higher arithmetic" at twelve years of age, and the next year I took up algebra as a little variation. This was not taught in the public schools, especially in the country schools; but the teacher that winter in our school being a young man who had spent two years in college and was interested in algebra, suggested that I might like to take it up. I was the only boy in school who was interested, so I took it up by myself and worked at it pretty much as I pleased. It was

interesting to me, but I did not see at once any direct field for it. However, I soon appreciated that in some of my more difficult arithmetical problems, I had really been using algebraic principles in making certain necessary assumptions; and so I began to try out my algebra on some of these problems, with results often astonishing to me. Some of the most difficult problems from the purely arithmetical consideration, proved to be exceedingly simple by algebra, so I soon began to think that algebra was going to be a handy tool in connection with my interest in arithmetical problems. Later I began to see the explanation of many of the curious arithmetical relationships through application of algebraic principles to arithmetic. This added new interest to the curiosities of arithmetic which I had met with from time to time. Also, explanations of such things as short-cuts in arithmetic soon became evident by the application of algebraic methods. In other words, the algebra furnished a fairly simple explanation of many arithmetical facts that I already knew.

In my further study of mathematics it became evident to me that a knowledge of the multiplication table above the usual twelve times twelve would be quite handy, and so I gradually learned it up to twenty-five times twenty-five and later up to thirty-six times thirty-six. Here was the place where my memory training proved useful, and also my algebra; for the latter helped to fix certain arithmetical relationships in my mind. I found this higher multiplication table was of considerable help in mental computations, but I did not appreciate the full significance of this until I got into actual engineering work, years later.

I gradually acquired a certain knowledge of approximations, such as calculations by percentage variations. This at first was to me more of a mental curiosity than anything else; but I soon began to see some use for it, as I found that I could very often give quick approximations in arithmetical relationships where others had to go through tedious operations.

I gradually discovered that skill at mental computation was giving me a direct advantage over others. I noticed this particularly after I entered high school, and later, in college. In my engineering work in college, especially in anything of a scientific, engineering, or mechanical nature, where quantitative ideas were involved, I could beat anyone else in approximating a result. However, I did not appreciate, until years later that this could be turned into a very valuable asset. I believe that my ability for mental computation was my most pronounced trait when I entered college.

One occasion that impressed itself upon me during my high school period, was a trip I made to Cincinnati with my father to see the Cincinnati Exposition. This was open for almost one month in the Fall of each year. I was particularly anxious to see this on account of the machinery. At this Exposition, I saw a Brush Arc Machine with four arc lamps lighting a small exhibit. This, to my mind, was the most interesting part of the Exposition, and I returned to it repeatedly. Even at that time, I knew enough about electrical apparatus to know that this was an electro-magnetic machine and not a friction type, as one of my elder friends insisted at the time. He said the name "Brush" machine referred to the brushes, while I insisted that the brushes had nothing to do with

the name, and that they were simply a part of the circuit. This was the first electrical machine that I ever saw and therefore was most interesting.

On this trip, I also visited the pumping station of the Cincinnati Water Works and was so taken with the great pumping engines that I could hardly tear myself away. In my early days, I was particularly fond of watching engines in operation. When I was a small boy I was willing to travel long distances, even in a most uncomfortable vehicle, just for the privilege of seeing a steam locomotive in motion. However, it was the machinery of the locomotive, very little else, that was of interest to me.

After the completion of my high school course, my father wished me to enter a college in our neighborhood, about ten miles away; but after studying over the courses presented, I said that there was nothing in them that attracted me, and that I would rather not go to school at all than take up anything they offered. About this time, however, I accidently obtained a copy of the Ohio State University Catalogue. This was then a comparatively young school, having been opened in 1873. Upon delving into this Catalogue, I discovered that the course in Mechanical Engineering so completely met my ideas that I told my father I would go to that school or none at all, as I wanted Mechanical Engineering. His principal objection was that it was a so-called "infidel" school, this title having been given to it by the various denominational schools throughout Ohio, because the O. S. U. was undenominational. Very harsh things were told about the O. S. U., but I insisted that I was going to school to be a Mechanical Engineer, not an infidel, and that that was the only school that I knew about that offered a

His Parents.

course in Mechanical Engineering. My father finally agreed with me, being quite mechanically inclined himself, and took me to the school to make arrangements for my entrance.

I entered the Freshman Class at Ohio State University in 1883, without any credit for mathematics. I had studied algebra and geometry and felt that I was in a position to receive full credit for either of these, but I had never studied trigonometry, which was one of the requirements for the Freshman year, as the first Freshman Class in mathematics began with analytical trigonometry. The professors refused to accept me, saying that I could not make it. Somebody advised me to go to see one of the senior students who was noted for his "horse sense." I did so and he told me that if I just kept still about my mathematics and went ahead, I "could probably make it, all right," especially if I did some preliminary work in trigonometry before the class opened. This was on Thursday; the classes began the following Monday. I worked day and night for four days on trigonometry, using my algebra to a very considerable extent, and felt by the following Monday that I was in a position to go ahead.

One of the freshman students came in to see me at the end of the four days, to have a "little fun," as he said afterwards. He had heard that I was trying to make up all the high school trigonometry in three or four days and he thought it was a good joke. He picked up the textbook and questioned me about various things. I told him what I had done and how I had gone at it and, after a little visit, he complimented me and went away. His roommate told me afterward that he had gone back to

his room and said that I knew more about trigonometry after four days than the fellows in his high school class knew after three months. He seemed to think that it was an almost unbelievable thing. He apparently did not appreciate that my good practice in algebra had been of great help to me.

The following week I went right ahead with the advanced trigonometry without the professor's noticing the difference, and the only comment he ever made on my work was the remark, from time to time, that I had evidently been over the whole subject before.

In this class in analytical trigonometry, there was one curious incident which I sometimes relate. The professor had a simple trigonometrical expression, which he wished to transform into a different expression. The initial equation and the final one were very similar in form; but apparently, in his mind, passing from the one equation to the other was a very complex operation; accordingly he filled a large blackboard with equations, to obtain the desired result. He then told the class to follow the work carefully, as he might ask some of them to repeat it the next day.

As I was looking over the equations that evening, the first thing that occurred to me was that as the initial equations and the final one were so similar in form, there should be a comparatively simple way of passing from one to the other. After a little study, it was obvious to me that by adding a certain numerical quantity to one part of the initial equation, it would give one term of the final equation and by subtracting the same numerical quantity from the other part of the initial equation, the remaining part of the final equation was obtained at

once. Of course, as the quantities added and subtracted were equal to each other, this did not modify the initial equation, but gave the desired result at once.

The next day the professor called for volunteers to reproduce the previous day's transformation. The only girl in the class, who was really a very good mathematician, got up and reproduced the general work of the day before. Other volunteers were called for and a self-important young man got up and reproduced all of it, with a great flourish. The professor was quite outspoken in his praise of these two. Other volunteers were then called for and as no one else seemed ready, I got up and presented my little simplified solution, occupying only two short lines on the blackboard. The professor looked at it and "snorted." He gave me no credit, but rather left the impression that it was not original with me; that I had been over the subject before and had gotten the solution from some one else. However, the majority of the members of the class gave me full credit. From this incident, I began to see, what I have found since, that the spectacular in mathematics usually brings more credit than simplicity.

My ability to carry relationships in my head proved to be of vast help in trigonometrical and more advanced mathematical subjects. In college I worked out many things in my head and was able, frequently, to give quick results in mathematics, but in most cases, I got credit only for things "coming easy" to me. Apparently, because people did not see the mental processes and the work involved, they thought no effort was required. But in fact, mathematics, in general, did not "come easy" to me in college, for I actually put in more hours per day

on such subjects than many of my classmates; and it took much mental effort to grasp certain factors in mathematics. One of the difficulties apparently was that I tried to get a physical conception of some of the mathematical ideas and relationships. This is not easily obtained, in many cases, but if once acquired, is of great use.

I did a considerable amount of my work in mathematics while I was out walking for exercise. I used to take long walks out into the country at times, and, not infrequently, I worked out some of my harder mathematical problems in my head as I went along. This was slow and tedious work, compared with paper solutions, but was very good training. However, when I came back to my room, I could often take up the problem, perhaps at the request of somebody else for assistance, and could work it right through from beginning to end, including all the equations, thus getting more credit for "things coming easy." I also acquired the practice of setting aside certain problems and solving them some time later, even after general interest in the subject had abated. This, I found very few of my classmates could do. I have found this faculty for taking up old problems to be of very great value in my later work.

I got so practiced in this mental computation that I could picture a problem in my mind with full diagrams and could produce the necessary equations and carry them through to a final result without touching a pencil to paper until afterwards. Visualization and physical conception of problems and principles were also carried into physics, mechanics, etc. This has probably been my most useful faculty in all my engineering work.

This faculty for mental computation apparently has

been closely associated with the ability to analyze, since early childhood. However, I carried this analytical ability or skill into subjects other than mathematics. Even in my early boyhood days I studied a good many of my schoolmates and knew their distinctive traits. In fact, when I was a small boy, my father used to laugh at times over some comment I would make about some of the neighbors' characteristics. I did not appreciate until after years that I was "hitting" some personal characteristics that most of the younger folks were not supposed to have knowledge of. I used to tell my father that certain animals around the place reminded me of certain people and would give him my reasons. He never said much but he laughed a good deal about it. Later I learned that some of these things were pretty close to the truth.

In these boyhood days I made also what later proved to be interesting discoveries regarding many of my associates. In some way, I got into the habit of thinking about their characteristics and discovered that I could predict some things which they were likely to do from time to time. I found that in some cases, I could even determine some of their characteristics from their appearance and conversation. For instance, in one case, which I now recall, I stated positively that a certain new boy in the neighborhood was "crooked all through." I gave as the only reason for my opinion that this characteristic stood out plainly in everything, even in his personal appearance. I was severely scored for expressing such an opinion, but within less than a month, the boy "skipped out" with the money of some people who had trusted him absolutely. Even at that time, while knowing practically nothing of psychology, I could not conceive how anybody

could be "taken in" by that boy, as his "crookedness" was so self-evident. I have found this early habit of studying other people's characteristics of much value in later years, especially in connection with my associates and assistants, and the selection of men for various kinds of work.

Also, I seemed to have a decided critical sense along with the other traits. For instance, when I was a small boy, I used to discuss with the older neighbors their pet superstitions, and then make a mental note of them and set about to check them from time to time. I soon discovered that many of their pet notions were purely imaginary and I discovered also that it was altogether useless to go to them and tell them so. I discovered also that in many cases, when the average man thoroughly believes in a thing, he does not want to find out whether he may be wrong, and does not want to be checked up in any way. I learned many of these things as a boy and found them of much use in later years.

Evidently, this analytical or critical ability gave me a certain standing in some of my college work; for, not infrequently, boys from other classes came to me to discuss subjects which were not at all within my line of study. Latin and Greek students even came to me to discuss the meanings of some of their translations. Such cases were always somewhat of a puzzle to me. To mention an example; in my Sophomore year, a man in the Junior class came to me with his text book on metaphysics to have a certain paragraph explained. I told him that I did not even know what "metaphysics" meant. He said that made no difference,—all he wanted me to do was to read the paragraph in question and tell

Portrait at 20 Years.

him what it meant. I read it and said that it evidently meant just what it said. "That isn't what I want; you just tell me what it means." So I explained to him what I thought was the idea intended. "That is just what I want," and he went away satisfied. I was not much surprised when such things happened in mathematical subjects, but it certainly seemed out of place to bring other radically different subjects to me.

In college I did not always receive proper credit for my ability for mental computation. For instance, the head of the Mechanical Engineering Department had taken a contract to do certain outside work involving rather tedious calculations. As I had assisted in part of this work, and as the men in charge were in such a rush when the proofs came in, the professor asked me to take various tables of figures and check them over carefully and as quickly as possible. Many of the quantities in these tables involved four or five figures. The professor gave me some four or five large tables to check up, one forenoon. I had classes up to the noon-hour; but from twelve until two I went over the tables carefully, checking the work over in my head. I did not check the figures individually, but compared consecutive numbers and checked their differences. With my ability for mental computation this was unnecessary, as the tables were prepared from formulæ, and the differences between consecutive numbers obviously all followed some fairly definite relationship, which I could note from inspection. In this way, I checked over the entire set of tables, finding a number of minor mistakes which I corrected, taking less than two hours, but missing my lunch for that day. I then took the tables to the professor, expecting

his praise for my quick work; but he "called me down" and said I evidently had not recognized the necessity for great accuracy. This irritated me somewhat, and I then challenged him to find any errors that I had overlooked. He worked for quite a long time on one of the tables without finding any errors other than I had already found; he then said that he would take it for granted that all the rest of them were right, but that he wanted to know "how in the world I did it."

Another trait which developed in childhood, one which has been of great use to me, is persistency. Often, as a small child, I would "stick things out" even if I had to force myself to do it. I would force myself at times to finish things even though the last stages were very irksome. I found this trait very valuable in arithmetic, algebra and other branches of pure mathematics. As I stated before, if I could not solve a problem in a certain time, I would lay it aside and take it up later, sometimes weeks and months later, and would eventually solve it, or at least reach some definite conclusion regarding it. If I did not grasp the principle or the explanation, I would often take it up again and again, sometimes a year or two later, and "mull" over it until I got a better grasp of the idea and could see it. This taking up of unfinished problems and ideas was usually done in my head, at odd moments, and was further practice in mental computation. This kind of persistency has been of immense value in later work, where I have solved certain problems only after ten or fifteen years of semi-occasional effort.

In going over some of these early traits it has appeared to me that all of them are fairly closely related to each

other, there being possibly only one trait back of them all. I am not enough of a psychologist to analyze these matters; I am simply telling them as they occur to me.

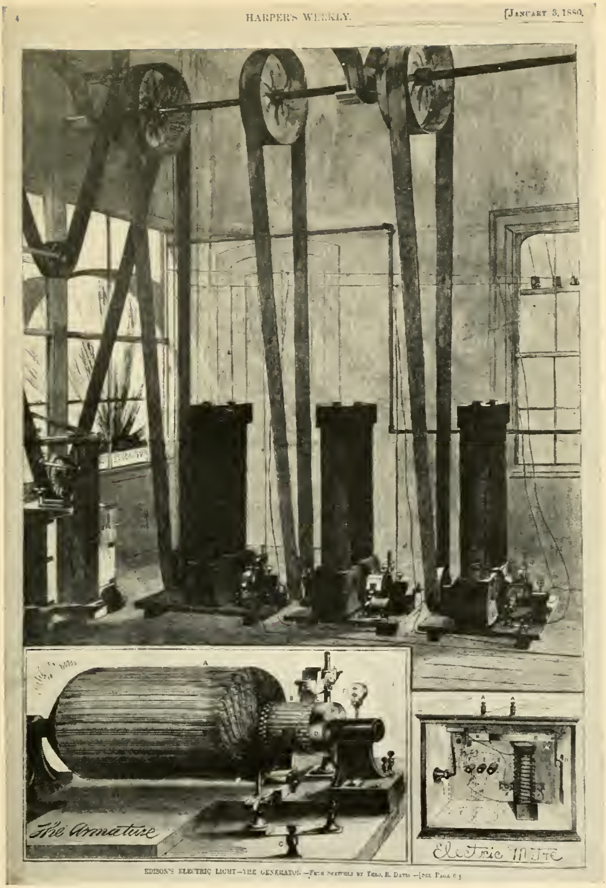
It was only after my leaving college that the value of the traits or aptitudes of which I have been speaking came to be appreciated. I was graduated from the Ohio State University in 1888, taking the degree of Mechanical Engineer. There were no electrical engineering courses in those days. However, I had been somewhat familiar with electrical principles, or electrical machinery, since 1880. About that time, as I said, I saw a Brush arc generator at the Cincinnati Exposition. I had read up enough on the subject from the books on Physics that I had come across to be able to recognize the functions of various parts of this machine. About the same time, I ran across a description and a picture of an Edison dynamo, published in *Harper's Weekly*.[1] This was only a reporter's description and the picture was very crude; but I studied them over and over, until I almost felt that I could build a machine, although I had no suitable tools or materials to make the attempt. Apparently I had got a pretty good idea of the apparatus, judging by certain things that came out several years later in my college work. For instance, in my junior year, I heard several seniors discussing an Edison machine, during some of their laboratory work. They had been working around this machine for several months. I then asked them several questions about the principles of the machine and its factors of construction and their purposes, and was startled to find that they knew almost nothing about the functions of the different parts, of which I had had crude

[1] See illustrations here and in appendix of this complete article.

knowledge for the past six or seven years. I did not understand how any one could work around a machine for several months without having some idea of the whys and wherefores of the different parts.

Looking at my college work, as a whole, I did not care much for the freshman year's work, nor for the sophomore's. During the junior year, it began to get interesting, as here we began to take up applied physics and mathematics in the form of analytical mechanics.

Prof. S. W. Robinson, who was at that time head of the Mechanical Engineering Department, was one of the ablest Professors at the University, possibly because he spent about half of his time on outside work. My earlier experiences with him, especially in the sophomore and junior years, gave me the impression that he knew vastly more than he really did. This impression gradually wore away, as I grew to know him, and through more intimate association, I obtained a still higher opinion of him. I discovered that he was thoroughly well grounded in a few fundamental principles and that he could use these in attacking, in a common sense way, all sorts of difficult problems. This was where I began to realize the importance of a knowledge of fundamental principles. I proposed, from time to time, a number of very puzzling problems, and I was astounded to find that, while, at first glance, he knew but little more than I did about them, he yet was able to pass very directly to the correct conclusion from certain fundamental knowledge which he always held available. Moreover, he took considerable pains, at times, to show me how to attack some of these matters myself; and I found, after a while, that in some cases, when I had the proper starting point, I could do



Early Apparatus for Edison's Electric Light.

This article had weight in interesting Lamme in electricity. Complete text is reproduced in Appendix, page 244.

almost as well as he did, in arriving at the proper result. However, in a good many cases I did not know the starting point. He used to tell me that there were two kinds of mathematics: "mathematical gymnastics" and "horse sense." He claimed that "horse sense" would handle most of the problems. I want to give credit here to Prof. Stillman W. Robinson as the best instructor that I ever had.

In my senior year, Prof. Robinson gave me considerable outside work to do in connection with work which he had undertaken. At one period, I spent all my spare time for several weeks working on the reconstruction of railway bridges for a certain railroad. I do not know whether anything ever came of this, but I do know that it gave me a sense of responsibility in the way of accuracy in calculation which I had never felt before.

Incidentally, I might mention also that I carried on this work in addition to my college work, although I was carrying something like one-third extra, above the regular course, owing to irregularities in the arrangement of my schedule in earlier years. This necessitated long working hours at night; and for about five weeks, I went to bed between two and three o'clock in the morning and got up promptly at six-forty. After the first few days, I became accustomed to this condition, so that, when Prof. Robinson would see me every few nights in regard to his work, I found that I could put him to sleep every time before the evening's work was over. Several times he asked me how I could do it.

It was during my senior year that Prof. Orton, then State Geologist of Ohio and head of the Department of Geology at the University, was authorized by the Legis-

lature to get out a new volume on the geology of the state. At that time (1887–1888) natural gas was coming to the front quite rapidly, and there apparently were no tables or data covering the piping of this gas under high pressures to great distances, such as 50 to 100 miles. Prof. Orton assigned to Prof. Robinson a chapter on the flow of gases, to form part of the new volume. Prof. Robinson, in turn, gave to me the work of making up the necessary formulæ and tables, together with all of the available data, which he was able to collect. He had results from actual tests made on long gas lines, and some published results of the air flow in the St. Gothard tunnel construction. I spent considerable time going over these data and trying to make something out of them. I finally told Prof. Robinson that our collected material was most inconsistent throughout, as some results were absolutely contradictory to others. I showed him, however, that by inspection I had obtained a general formula which fitted, with very considerable accuracy, practically all of the data, even some that were obviously absurd. I pointed out, however, that this general formula was absurd on the face of it, and that it was one of my evidences that all our available material was absurd. If I remember rightly, one part of my formula showed that under certain conditions the longer the pipe line, the greater the quantity of gas that would flow. I said I could not believe it. After studying my results, he said that he agreed with me entirely as to the inconsistency of the results and wanted to know how in the world I developed that formula.

In view of the conflicting results, he decided that we should have to start in and get new data. In the Mechan-

ical Laboratory of the University was a pipe line, several hundred feet long, supplying air to various forges, cupolæ and similar apparatus. So one morning, we started up the blower, tapped the pipe line at various points, and made a lot of tests with Pitot tubes. We discovered some interesting things in these tests. We found, for instance, that the air in the pipes apparently progressed with a rolling motion next to the surface; and we could get even negative pressures in our Pitot tubes at certain points. Such facts were probably well known before this, but none of our data at that time available had led us to expect such results. We then found that we had to take many and various conditions into account in determining the air flow through the pipe; and after a very considerable amount of analysis, it was found that reasonably consistent results could be obtained, that is, the calculated results agreed fairly well with our tests.

On this basis, I made up a series of tables based on certain formulæ derived from our tests. The results were published in the geological report which came out the next year. As an interesting outcome, I may say that several years later the head of one of the large natural gas supply companies notified Prof. Orton that the tables published in this geological report, covering the flow of gas in long pipes, were the only ones which gave them reasonably good results on their long pipe lines. I may mention that Prof. Orton gave me due credit for this work in the preface to his report which was published in the Ohio Geological Survey for 1888, I believe. Two years later, I heard from the State Geologist that this data had been accepted very enthusiastically by the gas people and that some of them had informed him that upon their

applying this information to the laying out of long lines the results had been very satisfactory, much more so than with any other data which they had been able to obtain. Naturally I felt much gratified over this statement, especially in view of the fact that the basic data, upon which the tables had been built, were relatively crude and, to a certain extent, inconsistent.

After graduation, in 1888, I left school for home, where I had to take charge of my father's farm for that summer and the following winter, until some suitable arrangements could be made for some one to take my place. In the fall of that year, I got hold of a copy of Sylvanus Thompson's "Dynamo Electric Machinery" and began to study it. I was usually too tired to study at night as I had to work until quite late. However, it was the practice in that part of the country for everybody to get up at five o'clock in the morning and do the chores before daylight. As I usually managed to get through with these about an hour before daylight, I would get up close to the kitchen stove for about an hour and read Sylvanus Thompson. That hour's reading was enough to keep my head busy for the rest of the day; for I could carry the illustrations and diagram pretty well in my mind and could study them out during my working hours. Often I went over the same matter the second morning and the third until I had got what I wanted. By the end of the year I had reserved a number of questionable points, with the view of submitting them, at some future time, to the professor in charge of electrical branches at Ohio State University. When I saw him later, I put my questions up to him expecting to receive direct answers; but to my surprise, he could answer none of them. The

only statement I got from him was, "You are now in a position to take the work up seriously."

I was much astonished and disappointed at the fact that I could not get the desired information from this professor; but a year or two later, when I got into active electrical engineering work, I came to the realization that neither the professor in question, nor any one else at that time, could possibly have given me the information I asked for, because most of it was not yet in existence.

In this connection, I remember, that in the latter part of my school days, I heard one of the engineers make some remark about "Westinghouse current." I made inquiry as to what he meant, but he could give very little information, with the exception that it was "A new kind of current which Westinghouse had gotten out." Of course, when I took up the study of Thompson's book, I soon learned that this was the alternating current system, which was brought out by the Westinghouse Company in America. This was the first time I had heard of the Westinghouse Company or of alternating current.

CHAPTER II

EARLY WESTINGHOUSE TEST ROOM EXPERIENCE

IT was the work which I did for Prof. Robinson on the flow of gas in pipe lines which got me into the Westinghouse interests. As I said before, after leaving school, I remained at home for the summer and fall. In the latter part of January, 1889, I ran across an article in a magazine or paper, describing Mr. Westinghouse's work in connection with the formation of the Philadelphia Natural Gas Company of Pittsburgh. It occurred to me that possibly my experience in preparing the tables for the Geological Survey would be of some value in connection with the Philadelphia Gas Company and so I wrote a personal letter to Mr. George Westinghouse, telling him what I had done. This was simply a "random shot," but, to my surprise, I received a note two or three days later, asking me to report at once to the Superintendent of the Philadelphia Company. As I had by that time someone to take my place at home, I went to Pittsburgh on the next possible train.

When I called upon the Superintendent of the Philadelphia Company, giving him my name, he immediately pulled out of his file my letter to Mr. Westinghouse and said that he knew nothing of the matter except that there was a pencil note on it from Mr. Westinghouse telling him to give me a job.

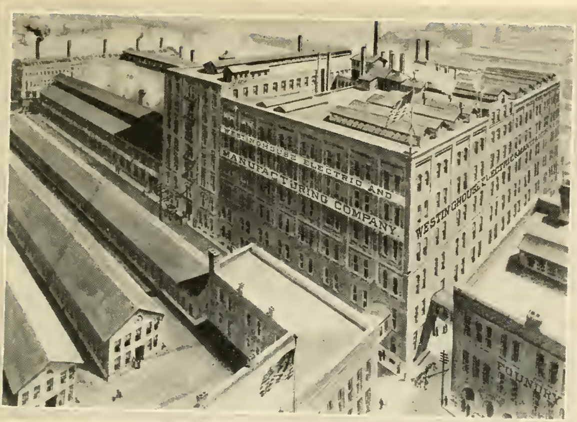
The work in the Philadelphia Company lasted only

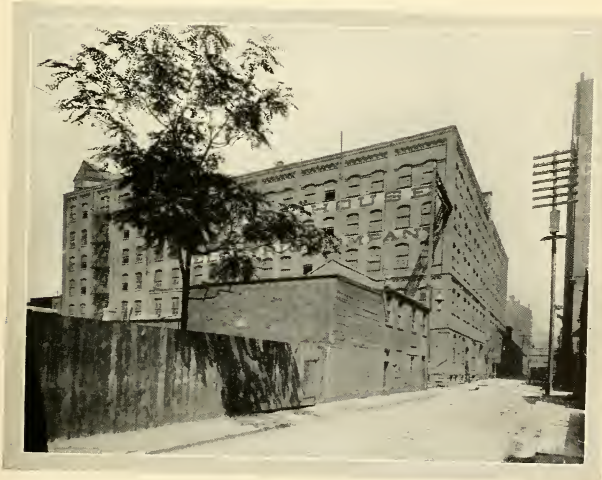
two months and was not the kind in which I was really interested. In fact, I felt that it was simply a job made for me at the request of Mr. Westinghouse. However, it gave me an opportunity to look around and study the situation, especially in connection with the Westinghouse Electric Company, which was the company in which I was really interested. Knowing two or three of the young men in the Electric Company, I took every opportunity to find out what the work was like.

About two months later, in a partial reorganization of the Philadelphia Company, the department I was in was disbanded and I was offered by the Superintendent work in an entirely different line. I told him that I did not care very much for that work, but was interested in the Electric Company. He said that if such was the case, he would give me a personal letter to the Superintendent of the Westinghouse Electric Company. Upon seeing the latter, I was told that they would take me into their shop as a beginner, at $30.00 per month. This seemed hardly enough to live on, even with the strictest economy, but I wanted the opening. I discussed the matter with several people, none of whom was any too enthusiastic. One man, who had pretty close connection with Mr. Westinghouse, told me it would be a mistake to go with the Westinghouse Electric Company, as there was no field there for an educated engineer. He said that all the work was being done by so-called practical men with little or no education, and he intimated that I would be a misfit. This was discouraging; but nevertheless I still wanted to work with the Electric Company and on the first of May, 1889, I asked the Superintendent of the Electric Company to place me. He asked me where I

wanted to go; in the shop, in the office, or in the laboratory. I thought he was joking about it and that he was just trying to sound me out to see what I would say. I told him that as I knew nothing whatever about the business and had to learn it from the ground up, I was willing to start where the chances of learning would be best. Then he took me out into the shop where we ran across a short, chunky, good-natured-looking man, to whom the Superintendent said, "Schmid, here is another boy for you." That was my introduction to the Shop Superintendent, Mr. Albert Schmid. Mr. Schmid took me to the testing-room and calling up another good-natured-looking man, said, "Fulton, here is another boy for your work." Fulton was the head of the testing-room, an ex-railroad engineer, and a very good-hearted, sympathetic man in his treatment of the college boys. This, my initiation into the Westinghouse Company work, was really the beginning of my engineering career.

My first duties in the Testing Department were to clean up the machines, polish all brass work, carry oil, see that all bearings were oiled and do other work of the kind. In the evenings, about quitting time, any new machines for test were started up, and, of course, I stayed around to see them. One of my first experiences with an electrical machine was in cleaning the collector rings. I was told to polish all the brasswork on the machine, without being told anything about the dangers. The machine in question was a rotating armature alternator of 1100 volts, having brass collector rings with brass brush holders and copper brushes. As, in telling me to clean the brass work, they did not warn me about cleaning the collector rings, I polished everything faithfully, in-





Two Views of the Old Garrison Alley Works in Pittsburgh.

cluding the collector rings, brushes and brush holders on the live machine. After I had the work finished, the man in charge came around and told me that I should, under no conditions, do any work around the collector rings and brushes, as it was excessively dangerous. I told him that I would be very careful not to do so, but did not tell him that I had already polished up these parts on one of the machines. This is the way we learned things in those days.

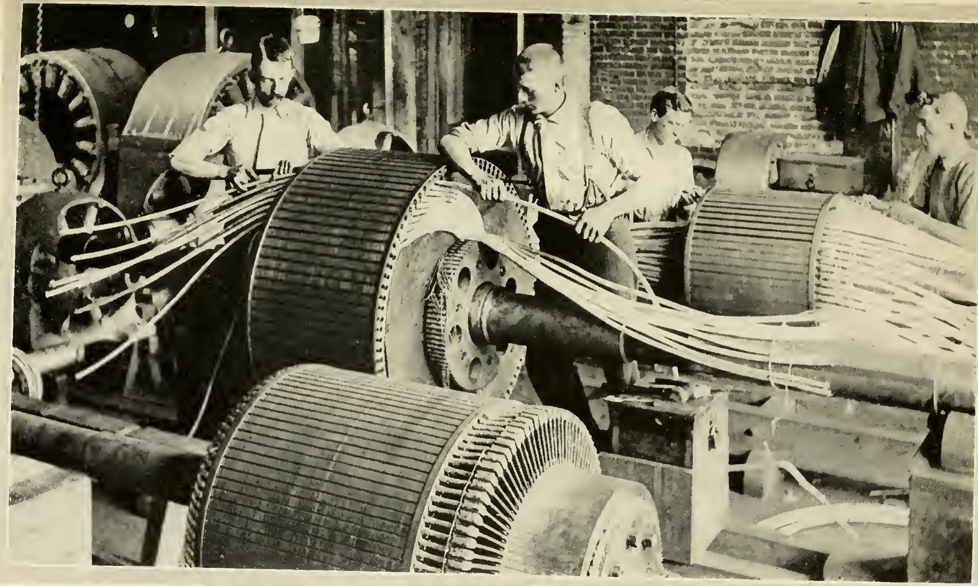
Shortly afterward, I received the first and possibly the most severe electric shock that I have ever had. There was a so-called testing bench with a large number of transformers on top of it, with leads running down through. I was told to clean up this bench, but was not warned to look out for the terminals, and I did not even know where they were located. The floor of the room was of wood with heavy spikes through to timbers buried in the ground. Not infrequently, there were grounds on the 1100 volt shop circuits. In consequence, any one working around the transformers might easily get a shock. In stooping over to clean the lower side of the bench, I received a terrific jolt, my first impression being that some one had given me a very violent kick. I looked around, but as no one was within reaching distance, I knew that it was something else. The shop testing man happened to come up and I told him my experience. He told me that I had evidently touched one of the terminals underneath the bench, and he showed me where they were. When I received the shock, I had been standing on one of the big spikes; and as one side of the circuit was grounded, I received an 1100-volt shock. The testing man then told me of the dangers and pointed out a num-

ber of the things that I should keep away from. This was my first instruction regarding the dangers of 1100 volts.

It may be of interest to say, at this point, that since that time I have received only one severe shock and that it was due to someone else's mistake, not my own. With that one exception, in all the years that I have been dealing with electrical machinery I have not received any shock that I could really consider highly dangerous.

The work in the test room was very amusing in some ways, as I now look back at it. Standard alternators were surface wound and banded. When a new alternator was started up, as was usually done about six o'clock in the evening, everyone got behind posts and other protecting places until the machine was brought up to full speed, and the field charge thrown on. The reason for this was that if the armature winding proved defective, it was sometimes torn into small pieces which would rain all over the place. The destruction of an armature winding in those days was quite an exciting event.

My general duties during this period in the test room consisted principally, as I have said, in oiling bearings, cleaning and polishing the machines and shafts, sweeping the floor, carrying oil, and doing any other work such as the boys were expected to do. We had also to help connect up machines for test, but this took only a small part of our time. One of my special duties was to take care of an 18 hp. vertical engine, located in the armature winding room next to the test room. This had to be looked after about once an hour, and started and stopped at the beginning and end of working hours. As this and my other duties did not keep me very busy, at times I



Early Method of Winding Large Direct Current Generator Armatures.

spent a considerable portion of my time in the winding-room, going over armature windings and worrying, by the multiplicity of questions which I asked him, Alexander Taylor, later Manager of Works, then a. c. generator winder. With an armature swung in a sort of a lathe, he stood at one side of the armature, winding by hand, turn by turn, while I would sit on the other side of the lathe on a high stool, which I had appropriated, and ask him all kinds of "fool" questions about it, many of which, of course, he could not answer; but at least we got well acquainted. This was probably where I got a pretty good hold on the underlying principles of armature windings, which have been, to a certain extent, one of my specialties.

I worked very hard the first month and put in considerable overtime, with the idea of getting some extra money, for I knew that some of the regular men got paid for overtime. I even worked several nights in addition to my day work. At the end of the month, however, I received only $30.00, being told that as I was only a beginner, or an apprentice, so to speak, I got no pay for overtime. This was a great disappointment.

It was not an unusual thing, when work came on at night, to ask one of the day men to stay over the night. This happened to me once or twice, and I then found that if I slept the next day and came around the next evening, I was liable to be assigned for a second night; this would continue through the remainder of the week until Saturday night, when there was no work; thus, I would begin new on Monday morning. Finding that this was going to lead to a great deal of night work, I developed a scheme for breaking the combination. The next time that I was put on night work at the end of a full day, I

stayed all night and also all of the next day. Toward the latter part of the following day, the foreman was going to ask me to go on night-turn again, but apparently recalled having seen me around all day, and asked me if I had not been on the night before. I told him I had; he asked me if I had not also been on all the day before, and I again told him that I had. He then wanted to know if I hadn't been on all that day too. Upon receiving a reply in the affirmative, he could not put me on the next night; so I stayed on day work all the rest of that week.

A rather amusing incident occurred in connection with my early work in the testing room. Once a day each of us had to make a little report regarding our work of the day before. I sat down at Mr. Fulton's desk behind the switchboard and wrote out my report in the morning. On one such occasion while I had a small piece of paper in front of me, without thought of the matter, I wrote down an equation at random, with the sign of integration in front of it. I was staring at this while thinking of something else, when I noticed someone leaning over my shoulder. Looking up, I saw that it was Mr. Fulton, the foreman. I thought he would scold me for being idle, but apparently he was much interested in what I had written on the paper. He evidently recognized the sign of integration, for he asked me rather eagerly,—"Did you ever study calculus?" I told him that I had and he said, almost reverently—"I would give anything in the world to have studied calculus." Apparently this little incident gave him great respect for me, which turned out very much to my advantage, for he apparently made somewhat of a pet of me, in

the sense that he gave me the easier tasks, and even would not put me on for a single night's turn, except when I expressed my entire willingness to go on. In fact, he favored me to such an extent that the other boys soon noticed it and wanted to know where I got my "stand in." I did not dare tell them, because a number of them were also college graduates and had studied calculus. This story I have told repeatedly as an instance of the value of studying calculus.

After about four months of this work, I was offered a position in the Incandescent Lamp Testing Department. This was night work, and hot work, too; but they offered me $40.00 instead of my former $30.00 a month and I needed the money. This work was from six in the evening to six in the morning, my duties being to watch the lamps and label each one with the date on which it burned out. I did this faithfully. However, every time the watchman came around on his hourly trip, he would remark something about my "being awake yet." I did not understand this remark until long afterwards when I learned that all the preceding men on this test had slept most of the night and labeled the lamps in the morning. After a few weeks of this work, I became ill, and upon consultation with the doctor, was told to hurry home at once, which I did. Upon arrival at my home, I was put to bed, having typhoid fever, and was not on my feet again properly for nine weeks.

During my first few months in the testing-room, I asked many questions of the few technical men I could get in touch with, regarding the reasons for certain constructions of the machines; but I could get very little information. I also asked questions of the laboratory

men, but could get practically nothing. I often wondered why all the work was done experimentally, and why calculation was not used to a greater extent, and even tried occasionally to do a little figuring on my "own hook"; but having practically no data or tests upon which to base figures, I did not get very far. However, I discussed such things with the other boys in the testing-room and elsewhere and this evidently made some slight impression, for some of them passed the word on to others.

Some months later, after the attack of typhoid fever, I returned to the shops one night just to see things again. Mr. Schmid and one or two of his assistants were there; Mr. Schmid spoke to me, which was rather surprising, as I did not know that he really remembered me. A few minutes afterward, one of the men with him, whom I knew, came to me and said that Mr. Schmid wanted to see me in his office the next morning. When I went to see him the next day, he said that he understood I could "figure things." I told him that I had done figuring in my mechanical engineering work in school, and that I had done some outside work for some of the college professors. I stated, however, that I had had no experience in "figuring" electrical work. He said that made no difference, if only I could figure. He then told me that he had been looking for a man to do some work on calculating electrical machinery and said that he had felt, for a long time, that electrical machinery ought to be calculated and the results predicted just as well as they were in mechanical design; and that he had tried several young men, but that none of them had produced any results. He said some of the boys had told him that I

Albert Schmid.

could figure pretty well, but when he inquired for me he found that I was away on account of sickness. Apparently when I came in, the evening before, he recognized me and that is why he asked for me.

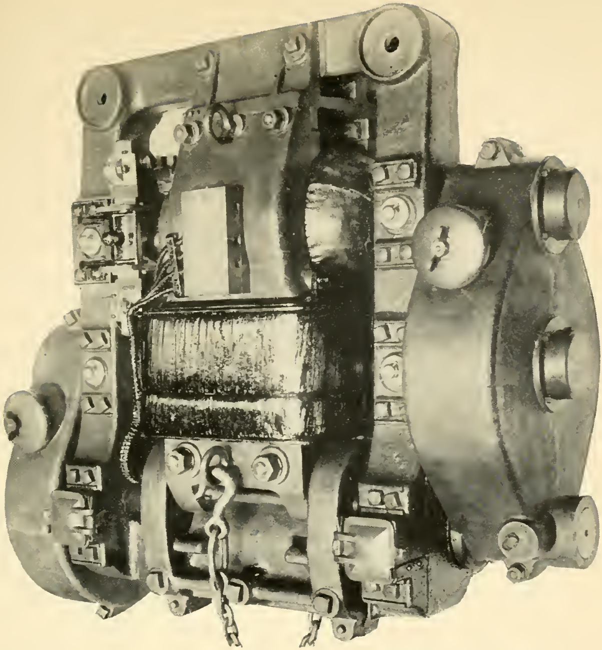
I told him that I was willing to make the attempt at figuring machinery, and would do the best I could. He then said he would turn over to me whatever tests he had and asked if I would try to produce the test results by direct calculation. He gave me some literature which he had collected on the subject of calculation of electrical machinery. He gave me space in an ante-room and told me to go to work on the figuring whenever I had any spare time from the testing. He also told me that he would give me $50.00 a month at the start, which was most encouraging after my previous experiences.

This was the beginning of my calculating work for the Electric Company. I worked pretty steadily, both day and night, at this work, as I found opportunity, and in a few days' time was able to produce saturation curves which checked fairly closely with the tests, considering that most of our data were only approximate. I was kept at this work about a month and he then said he was so much pleased with the progress I had made that he would raise me to $70.00 a month. That was a happy day for me.

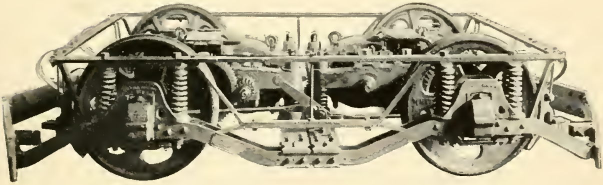
Shortly after this, in the Fall of 1889, he told me confidentially that Mr. Westinghouse was going into the street railway field and that he had asked Mr. Schmid himself to get ready as soon as possible. Mr. Schmid told me to get out around town as much as possible and examine all the existing railway apparatus in the Pittsburgh district, and look into all other direct current

machines that I could find. He also told me to collect all the data I could on the characteristics of such apparatus, with a view to seeing whether I could check up these characteristics by calculation. As I was able to do this fairly well, he set me to calculating a double reduction gear railway motor for the new Westinghouse system. This machine designed and built from these calculations, proved with but little modification, to be sufficiently close to the mark to be adopted as a standard. This was the first Westinghouse machine actually produced from calculations.

Meanwhile, I still kept on with the testing-room work. There were two testing rooms at that time, namely, the alternating current room and the room for arc machines both direct and alternating current; later, the street railway testing room was added. Most of my time was spent in the alternating current testing room. I suggested various minor improvements on this alternating current apparatus, including a method of compounding the alternators. Mr. Schmid had brought through a toothed type of alternator, which, however, required a special design of laminated field, whereas, the standard alternators had cast iron poles. This toothed armature did not work in these cast iron fields without excessive heating of the field structure. After studying the matter, I suggested that the shape of the armature teeth be changed considerably and that the machine be made with a very large air-gap, as large, in fact, as that of the older standard face-wound type of machine. Mr. Schmid told me to go ahead and try these changes; and when I did, it was realized, at once, that a very great improvement in heating was obtained, so much so that the output of the



Early Double Reduction Railway Motor.



Truck Equipped with Double Reduction Railway Motors.

Shows comparative complication over later single reduction motors illustrated opposite page 48.

toothed machine could be run up to 50% above that of the corresponding surface wound machine. This was an extremely promising result, except that the regulation of the machine was relatively poor. I then applied the method of compounding, mentioned above, and showed that, with the addition of this compounding, the full 50% increase in rating could be obtained. Mr. Schmid was exceedingly pleased with this result, for his toothed armature construction had been under fire, so to speak, and these changes took it out of the questionable type and made it a thorough success.

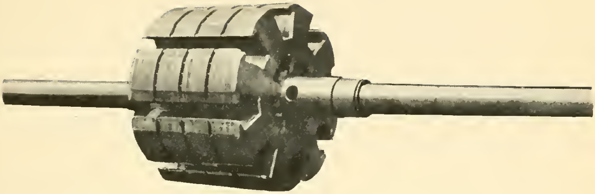
This result was so revolutionary that it immediately put the toothed armature construction in the lead; and, from that time on, the surface wound machine was built in connection with repair work only. This was the first revolutionary step with which I was connected. The credit for this new type of armature belongs entirely to Mr. Schmid, but its successful application to the old lines of machines with cast iron poles was due, to a considerable extent, to my efforts. After this accomplishment, I was given more leeway than ever in connection with the test-room work, Mr. Schmid backing me to the limit in practically everything that I wanted to undertake. A complete line of toothed armature alternators was developed, and this general type soon became the accepted standard of the whole country.

In view of this and other work which I had been doing in the testing room, Mr. Schmid then turned over to me all alternating current testing, including development work in the testing room. Meanwhile, I had been getting out the designs for the new railway motor, as I have said, and was keeping in pretty close touch with the arc

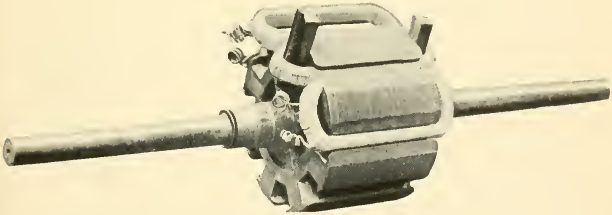
lighting work, covering the Stanley alternating current and the Waterhouse direct current machines. I helped with the experimental work on this apparatus, but was not in direct charge of it.

This work kept me pretty busy. Much of our special testing was done at night and I felt that I had to be on hand up to midnight, or on many occasions even later. I also had to be present during the day-work, as I had charge of one of the testing rooms. Frequently, I left the shops at midnight coming back again at seven o'clock in the morning. All of the watchmen and most of the foremen knew me personally, although some of them knew only my first name. I could come and go as I pleased; nobody kept any track of me, because they felt that I would usually be around if anything happened.

Several months after I had been put in charge of the alternating current experimental testing, the man in charge of the arc testing was sent out on a trouble job and I was told to take care of his work while he was away. Thinking that this was only temporary, I did not say much about it, although it meant more work. When I had handled this extra work several months, the man in charge of the new railway motor testing section was also sent out on a trouble job and I was told to take care of his work until he came back. This meant still more work and still longer hours. I objected a little bit this time, and was told by Mr. Schmid to go ahead with it the best I could until the others came back. After several more months, when they had not returned, I "kicked" again; but Mr. Schmid just laughed as if it were a joke on me, and did not give me any answer. I did not realize until long afterwards that he had not intended the



Core of Toothed Armature of A. C. Generator.



Method of Placing Coils on Toothed Armature of A. C. Generator.

others to come back; that it was only his way of turning the whole testing work over to me. Presumably he felt that after I had got deep enough into the work I wouldn't give up, but would "stick it out."

Another practice in those early days was to transfer other young engineers from the testing room to "road-work," as it was called, this being looked upon as a big step forward. Man after man was sent out until all my original associates were gone and I was left to do most of their work. After awhile I went to Mr. Schmid and told him that all the others had been given a chance "on the road" except me; and I wanted to know what was the matter with me. He simply had a good laugh over it and gave me no satisfaction. Much chagrined, I went back to my work. It was not until long afterward that I began to appreciate that he was purposely holding me in the shop; but, after I had got well into development work, I very much liked the idea of being held in the shop; even being afraid, by that time, that I might be transferred elsewhere.

It was in the summer of 1890 that I began to expand on design and development work. My work in calculation had so far developed that I was doing more than checking existing machines. I was beginning to predict new types and designs. My first radical departure from existing types was in connection with the single-reduction railway motor, which was a direct result of my methods of calculation.

Mr. Westinghouse employed Mr. W. L. R. Emmet, the now well-known General Electric engineer, to help with the development of his early railway system. Mr. Emmet had had experience with the Sprague equipments and was

especially familiar with the control part, which was entirely new to us. The first motor equipment having been tested out under my supervision, was installed on a car of the Pleasant Valley system in Allegheny City, then separate from Pittsburgh. In mounting the first equipment, Mr. Emmet and I spent many days and evenings together going over all the details of the situation and trying to anticipate possible "bugs." At that time, he gave me the benefit of practically all of the experience he had had with the Sprague Company. This was a very considerable help to me, as it covered a part of the work with which I had had very little chance of becoming familiar.

CHAPTER III

EARLY MACHINES BUILT FROM PRE-CALCULATED DESIGNS

IN August, 1890, during a strike at the Company's plant, the test room was shut down, and to keep busy, I took up the problem of the electrical design of a single-reduction-gear motor for railway work. My short experience with the double-reduction motor in service had convinced me that this type, with its exposed armature and field windings, and its two sets of gears, could not persist very long. Consequently the production of a new type of motor to overcome these difficulties was at that time a very live problem.

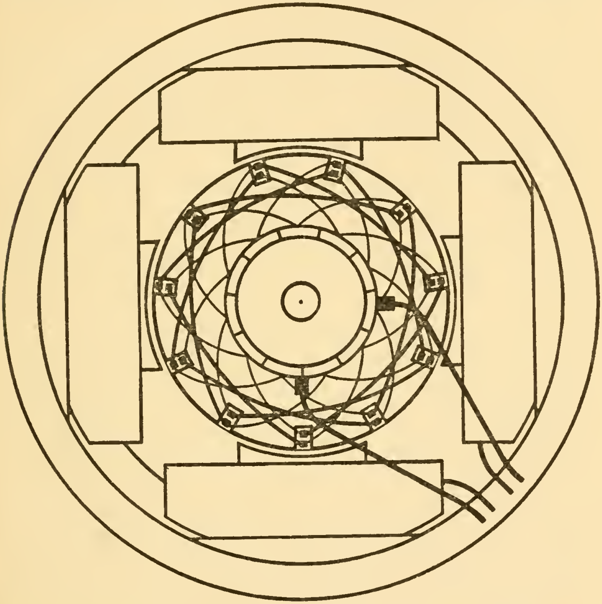
My little practice in calculation doubtless having given me undue confidence in tackling this new problem, I deliberately undertook to analyze its possibilities. Right at the start, my rough figures indicated that such a machine should be of a four-pole type in order to keep down the weight. With this as a starting point, I naturally turned toward the Westinghouse alternator type of field construction with its internal, symmetrically spaced poles. This appealed to me as presenting several great advantages for railway motor construction, for the external yoke of the machine naturally formed an ironclad protection for the motor as a whole; and it would be a very easy matter to house the lower half to protect it against damage from below. So far, so good; but when

it came to the armature, I was fairly "stumped" for a while.

I first worked out a surface-wound armature, such as was common practice on railway motors at that time; but I found at once that, owing to the large iron-to-iron gap, the construction was practically prohibitive with four poles. This, apparently, brought me up against a blank wall. However, various other possibilities were then considered, among them the "slotted" type of armature, such as was being used on small capacity machines by the United States Electric Lighting Company, then a subsidiary of the Westinghouse. This type of armature was considered practicable up to a few horsepower, but no one imagined it to be at all suitable for anything as large as a railway motor. But, as I was strictly, "up against it," and was, therefore grasping at straws, this slotted construction was given serious consideration. To my great pleasure, the first figures indicated that, from the magnetic standpoint, the slotted construction allowed a comparatively low speed motor of proportions permissible for a street car equipment.

But when it was attempted to work out the armature winding itself, another most serious difficulty appeared. Up to this time, the one type of winding considered practicable for four-pole machines was what is now known as the "parallel" type with four circuits, requiring four brush arms unless all the commutator bars were cross-connected. Since this appeared to me to be impracticable for railway work, we were faced by a most serious difficulty. Nevertheless, I then spent several days attempting to work out some form of four-pole armature winding which inherently required only two brush arms, and had

only two circuits in the armature instead of four. After two or three days' work, I found a winding which gave exactly what was desired. This was used in the construction of the first motors of the new type. I still have



Typical Diagram of Two Circuit Armature Winding as used on First Single Reduction Railway Motor

my old pencil sketch, dated August, 1890, showing what was almost my first "picture" of such a motor. This shows a 63-slot armature with 63 commutator bars, and six turns per coil, with a drum type armature. I soon decided that this did not give enough commutator bars

and changed to a 95-slot armature with four turns per coil, giving almost exactly the same total number of turns as in my earlier sketch.

When I had reached this point, I took my crude results to Mr. Schmid. He received them very enthusiastically and immediately had drawings begun, the Company's draughting room being directly under his charge. He included a number of important features, principally of a mechanical nature, and arranged to have two machines rushed through for a trial equipment. To avoid attracting attention, the new machine was not called a street railway motor, but was designated a "mining motor"; and as no one knew what a mining motor should look like, no curiosity was aroused and but few questions were asked.

After the work had well progressed, it dawned upon Mr. Schmid that an entirely new type of armature winding was specified in this machine, in addition to all of the other radical features. When taken to task I explained to him why it had been used, and insisted that it was theoretically correct, and that there was no other known way of accomplishing the desired result. As the work had progressed so far, he accepted my arguments. Another variation from previous practice in these first two machines, was that $\frac{1}{32}$-inch mica was used between the commutator bars, whereas the usual practice on railway motors at that time was $\frac{1}{16}$- to $\frac{1}{8}$-inch mica. This radical departure, it developed afterwards, was one of the exceedingly fortunate things that we had done in making this machine.

As originally planned, the two new slotted armatures were to be hand-wound, as hand-windings were standard

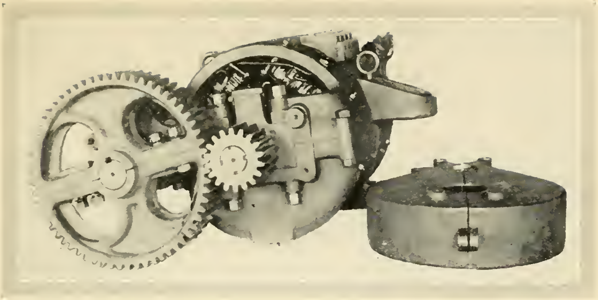
practice at that time. Shortly after this was tried, I became thoroughly convinced that the hand-winding for the slotted type railway motor could not be entirely successful; accordingly I then proposed machine-wound coils, insulated before being put on the core. My associates and I then spent two or three weeks in attempting to produce machine-wound coils of such shape that they could be placed symmetrically all around the armature core, according to the method developed in later practice. We very nearly succeeded in this. Later, it appeared that, with certain minor modifications in the shape of our coils, we should have obtained the symmetrical winding. At the time, however, as the work was in a great rush, it was finally decided to put on machine-wound coils, the ends of which were to be bent into position after being placed on the core. The foreman of our transformer winding department and I, neither of us with any previous experience as armature winders, then put the machine-wound coils on these two slotted armatures, both of which stood the shop tests and were sent out on the first car equipment. Even at that time, I considered this as most conclusive proof of the superiority of machine-wound coils; that two inexperienced men could make a success of this winding in their first attempts.

In the latter part of 1890, when these two machines were put on test, they checked quite closely with the calculations; and, what was more important, the slotted armatures commutated better than we expected, the new type of armature proving to be thoroughly satisfactory. This motor, in the spring of 1891, was put on the market as the Westinghouse No. 3; and, so quickly did it take, that the entire stock of double-reduction motors on hand,

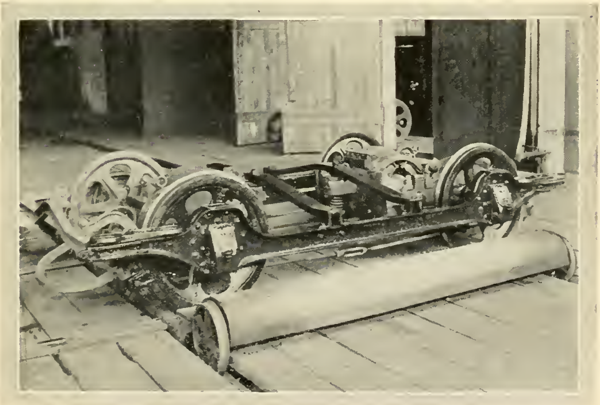
some two or three hundred in number, had to be scrapped, as they could not be sold.

This single-reduction-gear motor should be looked upon as the direct "ancestor" of the modern universally adopted type of railway motor. In fact, the present street railway motor, with certain additions and improvements, contains practically all of the main features found in this early railway motor. Moreover, the new type of armature winding, which later became known as the "two-circuit" or "series" winding, is now used the world over. The pride which I feel in my part of this early development is not so much in the motor itself, as in the type. When it is considered that the life of most types of electrical machines does not average more than ten years, it may be seen that the initiation and development of a radically new type of railway motor, thirty-five years ago, which has since become the universal type, and, as yet, shows no signs of obsolescence, is something of which any one should have the right to be proud.

This was something which gave me confidence. A radically new type of machine had been produced by calculation, and had showed a tendency to revolutionize the practice in an important section of the electrical field. This gave me the idea that other types of electrical machines could be improved, or even revolutionized, by calculation. From calculation, I then brought out a complete line of 133-cycle, single-phase alternators with toothed armatures, using Mr. Schmid's toothed armature construction. Also, about this time, the 60-cycle alternator was brought out as a standard, and I developed, by calculation, a complete line of single-phase 60-cycle, toothed armature machines. This made me feel



Early Single Reduction Railway Motor.



Truck Equipped with Single Reduction Railway Motors.

Shows gain in simplicity over earlier double reduction motors illustrated opposite page 38.

that I was getting the work well in hand. I was still connected with the testing room, being considered, I believe, Foreman of the testing room, although the routine work was handled largely by others while I spent most of my time on calculations.

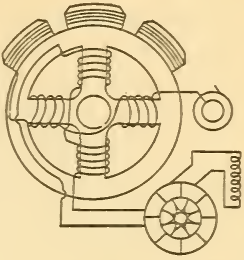


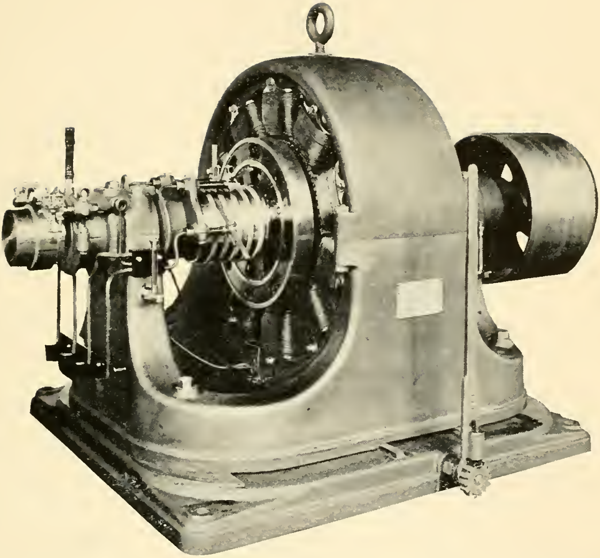
Diagram of Compensating Winding on Alternating Current Generator

During this same year, some very interesting work was done in the testing room. For instance, when operating an alternator as a synchronous motor, we noted that the generator voltage, when the motor was separately excited, did not vary up and down directly with the variation of the generator excitation. As an extreme test the generator field was opened and the synchronous motor continued to operate with about one-half voltage delivered by the generator. It was well known, at that time, that an inductive lead on the generator would lower its voltage, owing to armature demagnetization, although the action of such demagnetization was not so plainly understood as in later times. Here, however, was an exactly opposite effect and, upon observing this result, I said that this must mean that the current from the generator to the motor must be *leading* with respect to the generator, instead of lagging. I said further that if this suggestion were correct, it should be possible, by means of a synchronous motor, to exert a regulating action on an a-c. system. Further tests indicated that this opinion was correct.

At a later date, a patent application was filed, claim-

ing the governing of the phase relation of the current to the voltage in an alternating system, and covering the use of a synchronous motor for regulating the voltage on a supply system. Incidentally, I may mention that the Patent Office rejected this application on the ground that an alternating current could not be ahead of its e.m.f. Some time later, Mr. John F. Kelly, the well-known engineer, obtained a patent on exactly the same arrangement, in which he explained the action on the basis of current lag of greater than 90°, thus giving the effect, or the equivalent, of leading current. On this basis, he obtained a patent. However, as his scheme was exactly the same in principle as mine, which had been rejected, the matter was opened again and an interference declared among a number of inventors. It developed, however, that my filing date was earlier than the earliest conception of any of the others, and thus a broad patent was awarded. However, this scheme did not come into general use until so long afterwards that the patent had practically expired. I simply cite this as an illustration of some of the work we were doing in our test room at that time.

So much for the alternating current machines. From time to time we were testing a few direct current machines which were mostly of the Weston type. As the Westinghouse Company had acquired the United States Electric Lighting Company, most of the direct current machines were turned out for us by that company. Occasionally, we got one in for repair. At that time, the United States Electric Company was making direct current machines up to 200 hp. All direct current machines at that time were rated in horsepower.



Early Form of Compensated and Self-Excited Alternating Current Generator.

My first serious experience with any of these machines was when a 200 horsepower armature was sent into the Westinghouse shops from the Pittsburgh Reduction Company (now the Aluminum Company of America), for rewinding. If I remember correctly, this was a 60-volt generator, having two commutators, one on each end, and the armature being wound with flat copper strap about $\frac{7}{32}$ in. thick and $1\frac{1}{2}$ in. wide, lying flat on the armature core. The windings were connected directly across from one commutator to the other. The armature returned to us was roasted black. From what we could gather, this machine had been tested a number of times before shipment, by the United States Electric Lighting Company and had roasted out, each time, on test. This company finally succeeded in shipping the machine by sending it out without test. Consequently, as soon as the Pittsburgh Reduction Company ran the machine in its plant near Pittsburgh, it promptly roasted out again. This armature was then brought into our test room and I was asked to look it over and determine the cause of the trouble, with a view to rewinding it. At that time, knowing something about the effects of eddy currents on wide conductors on surface-wound machines, I pronounced the trouble to be due to the eddies in the wide straps. As a remedy, I suggested cutting a groove about 3 in. wide and 3 in. deep around the center of the armature core and then winding the armature with five insulated conductors, in parallel, crossing the conductors at the center groove, with a view to eliminating the eddies. This apparently remedied the trouble, for the reconstructed armature operated for two or three years.

In working on this, I evolved some more or less wild

theories as to how large armatures should be made. I told Mr. Schmid, incidentally, what I had been thinking about, but that was as far as I expected it to go. A few weeks later, he came down into the test room with a nice, jolly-looking man; calling me up, he introduced this man as Mr. John F. Kelly, Chief Engineer of the United States Company. I had often heard of Mr. Kelly and quite properly looked upon him as one of the "big" men of those times. Consequently, I felt very much pleased at meeting him. I soon learned the reason for the introduction. Mr. Schmid began by saying that he had told Mr. Kelly about some of the ideas I had proposed in regard to the construction of large direct current armatures; he asked me if I would repeat them to Mr. Kelly. I then explained that one serious difficulty, in the case of large machines with surface wound armatures, would always be in the eddy currents in the conductors; and that I thought the conductors should be sunk below the surface in holes or grooves. Furthermore, in the case of the Pittsburgh Reduction machine, I said that, on account of the large currents, there should be but one conductor per slot, as the commutation would thus be better; and that the number of commutator bars could be so arranged as to give smoother commutation than there had been in the former machine. I went into the matter quite fully, knowing that there would be very little chance of proving out my "notions." Mr. Kelly smiled a good deal, apparently over my enthusiasm, and then walked away with Mr. Schmid.

Imagine my surprise a few weeks later when I was called to Mr. Schmid's office and was shown the drawings for a new Pittsburgh Reduction armature, containing all of

the suggestions that I had made to Mr. Kelly. These had just arrived from the United States Electric Lighting Company and Mr. Schmid said that we were authorized to go ahead and build the armature. "The chickens had come home to roost." I was somewhat staggered over the matter, at first, but was "game" enough to go ahead. This was the first large slotted armature of the modern type ever built in this country. This machine was built soon after, but on account of the successful operation of the old reconstructed armature, already described, it was not put into service for about a year; and hence, was not the first big slotted armature put into operation, as our first slotted railway generator, described later, had been put on test in the interval.

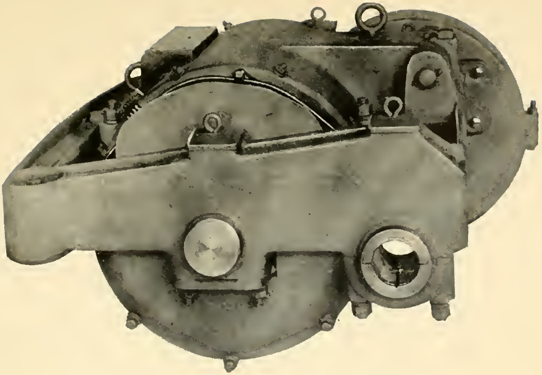
When I look back on some of the things that we did in those early days, with such very little data at hand, it is always a source of astonishment to me that the results turned out so well. If there is any such thing as good luck, we certainly had some of it.

It may be seen, from all I have said, that Mr. Schmid was a very broad-gauge man in many ways. He was always anxious to obtain information, especially in regard to principles. Very often I was "hard put" to explain things properly. He was of a most progressive disposition and wanted others to progress with him. He was also of a most helpful disposition in many ways, for, if men under him "fell down" in a laudable undertaking, Mr. Schmid would assume the burden of criticism or condemnation; but if the endeavor was a success, the men undertaking it received due credit. In time of trouble, he stood by his men and helped them all he could. In those days, people would come and warn me against giv-

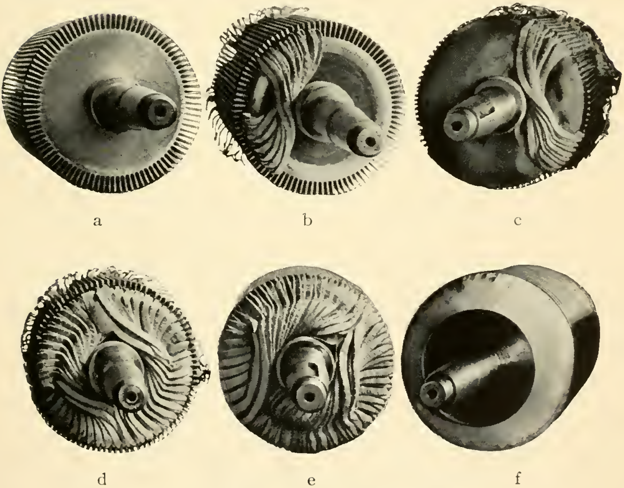
ing Mr. Schmid so much information. They told me he would "pick my brains and throw me aside," as he had done with others before me. I happened to know the real reasons why he had thrown certain others aside, and the reasons were good ones, in my opinion. In such cases I sometimes said, "If I am preparing information and data for Mr. Schmid, and he can take all I have and go ahead of me, he should throw me aside." The principal difficulty was that he was not severe enough with some of his men, and they took advantage of him.

In the spring of 1891, as has been said, the No. 3 railway motor was put on the market by the Westinghouse Company. This No. 3 motor was very nearly on the same line as our experimental motors already described; but one radical change was made which was decidedly for the worse. At that time, all railway motors were of the surface wound type and $\frac{1}{16}$ in. mica between commutator bars was the rule; sometimes it was thicker. When trouble developed at the commutators of such motors, there would be a call for still thicker mica. Consequently, when it was found that our original single reduction motors had $\frac{1}{32}$ inch mica, this was condemned utterly. We were told that such a machine could not be sold. In consequence, when the first order for No. 3 motors was put through, we went back to $\frac{1}{16}$ inch mica. Please remember that this was not the soft built-up mica of today, but the hardest kind of mica punched out of a single block and not even split up into laminæ. From our modern viewpoint, it is no wonder that trouble soon developed in the commutators of those No. 3 motors.

Mr. R. S. Feicht, now Director of Engineering of the Westinghouse Electric & Manufacturing Company, who



Early Form of No. 3 Railway Motor.

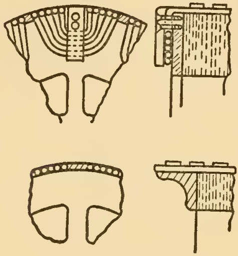


Armature Core of No. 3 Railway Motor and Winding in Progressive Stages.

was sent out in connection with some of these earlier motors, reported, in a short time, that the mica was "standing above" the copper and that the commutators were considerably blackened. As an obvious, though temporary, remedy, he adopted the practice of cutting this mica down flush with, or below, the surface of the copper. This kept the equipments going for a while, but it was recognized that this condition could not be permanent. Meanwhile, the first two experimental motors were operating right along with bright commutators and no high mica. Finally, I insisted that we should go back to the thin mica, on the No. 3 motors, citing the old motors as an illustration of the results to be expected. New commutators with $\frac{1}{32}$ inch mica split into thin laminæ were furnished on the No. 3 motors already out; with that, the high mica trouble practically disappeared. Thus, it may be understood why I said, in connection with the design of the No. 3 motor, that *fortunately* we used thin mica on the first trial equipment.

Up to the spring of 1891, the United States Company had furnished generators for the Westinghouse railway equipments. At this time, we received an order for a 250 hp. railway generator which was larger than anything that the United States Company cared to attempt in a 2-pole design. Therefore, a 4-pole machine was undertaken at the Westinghouse plant and this was put into my hands. The field frame of this machine was made of the 4-pole radial pole type, much like the modern design. The armature was of the 4-pole *ring* type, and *surface* wound. At that time, the ring type of armature was supposed to be the coming type. This armature had a bronze supporting spider.

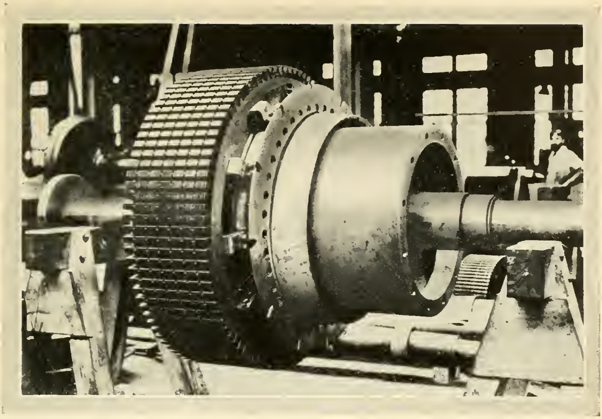
When it came to winding this machine, unusual difficulties were encountered; for the ring type was new to us, and the size of the machine was greater than anything undertaken before. After several attempts to wind by hand, a semi-formed coil construction was adopted, the coils being made U-shaped and taped, and then slipped over the core at one end, thus making a partially machine-wound construction. This was one of the first jobs assigned to Mr. N. W. Storer, now of our General Engineering Department. Regular winders did not think the construction possible, so we had to put on this work some men who were not so sure that it could not be done.



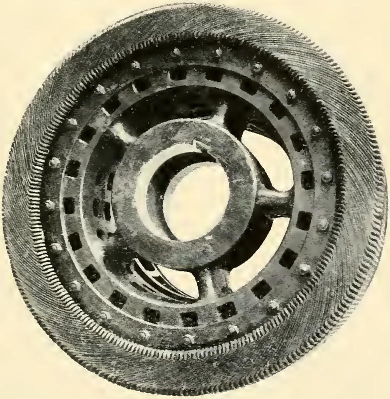
Method of holding Coils in place on a Surface Wound Armature

During the test of this machine, it apparently worked pretty well. Finally, a short circuit test was made, to try out a lightning arrester, I believe. This was the first "dead short" I ever saw put on a large d-c. machine and its effects were somewhat startling. A lot of people saw this test and were much gratified with the result. However, I was not so well pleased, having discovered that the armature winding had shifted two inches on the core. This bothered me a good deal,—in fact, it went a long way toward convincing me that the surface wound type would not do.

As soon as this machine was completed and out of the way, I discussed with Mr. Schmid the question of making a slotted armature machine for large railway gener-



Slotted Armature Core for a Large Direct Current Machine.



Form Wound Coils Placed in Slotted Armature Core, as Above, to Form a Complete Winding.

ators. I was fully convinced that we should have to depart from the surface-wound type. After he had had me figure on a machine, it was concluded that it was worth while to undertake a slotted armature construction. Such a machine was built in the summer of 1891, and while there were some peculiarities in its construction, it was so similar to some of the later types of slotted armatures, that it served to prove that the construction was the right one to follow. Tests on this machine were made just previous to a convention of the Street Railway Association in Pittsburgh that fall, and it was put through "its paces" for some of the visitors. It carried current from no-load to considerably above full-load without shifting the brushes, and when the whole load was thrown off suddenly, there was no serious flashing or sparking. This was considered a marvelous performance at that time, especially for the slotted type of machine.

Incidentally, I may mention that while we were constructing this machine, Mr. Schmid took particular pains to obtain the opinions of a number of well-known American and foreign authorities on direct current construction and they all, without exception, condemned this proposed armature construction as utterly impracticable. However, fortunately the machine had progressed too far by that time and so we decided to see it through.

This machine was revolutionary in its construction and the result sounded the early doom of the surface wound direct current type of machine. Moreover, this was also a turning point in the tendency toward the ring type armature. This first slotted type generator was made with the drum type winding, in order to use a 2-circuit type of winding most easily, and also to avoid the neces-

sity for a non-magnetic type spider. This, I believe, was the first large machine with the 2-circuit type of armature winding.

An interesting point in connection with this type of winding is that it was the general opinion among many engineers even several years later, that the 2-circuit type could not be made of symmetrical construction for other than four poles; and that for six or eight poles, it necessarily had to be unsymmetrical in some way. In working out this winding originally, it had become evident to me that it could be used for any number of poles if properly designed; and I did not hesitate to use it on six or eight pole machines, somewhat later, when slow speed generators came into use.

At the World's Fair, in the summer of 1893, several members of the Jury of Awards asked me whether the 2-circuit type of winding was possible, in a symmetrical form, for more than four poles. I assured them that it was; that we had already used it for six poles or more, and that I wanted to know why they asked the question. They said they had been definitely informed that such a winding was not possible.

This first slotted railway generator had many characteristics which we used in later practice, such as high saturation in the armature teeth, low commutation reactance, etc.

In connection with the multipolar type of railway generator, there was in those days one curious situation. It was claimed—and apparently nobody contradicted it—that multipolar direct current machines could not be run self-excited, although bipolars could be operated in this way. I, personally, looked into several instances

where separate excitation was used on multipolar machines, built by various companies. After doing some calculating, I came to the conclusion that the difficulty was altogether in the insufficient saturation of the magnetic circuits. These machines were of the surface-wound type of armature, with extremely large iron-to-iron gaps; the magnetic densities were extremely low, with a resultant lack of saturation. This was so apparent in the designs I examined, that I concluded that the fault in self-excitation was not in the principle, but simply in the construction; accordingly, when the new Westinghouse multipolar machines were put out, I put a fairly high saturation in the yoke, with the intention of making the machines self-exciting. The first machines were shipped and operated as self-exciting machines before it became generally known what I had done. Instead of being praised, I was severely criticized for attempting the impossible. My contention that I was right because the machines were working satisfactorily did not seem to carry much weight; for I had gone contrary to a well known fact, and therefore was in the wrong. Nevertheless, I kept on putting out self-excited multipolar generators, and the public was satisfied with their operation.

Another line of development which started in 1891 was the rotary converter. There had been much discussion about machinery for converting from alternating to direct current and I did considerable paper work to determine the possibilities; I even designed a 150 hp. rotary converter, in order to see what could be done. This machine was not unlike the modern type rotaries. Nothing, however, came of this at the time.

Up to this time, I had practically nothing to do with induction motor work. When I first came into the company, in the Spring of 1889, the factory work was being carried on by Mr. Tesla, assisted by Mr. C. F. Scott. I used to stand around and watch Mr. Tesla carry on his tests. However, in 1890, the induction motor work was abandoned for a time, largely because no suitable supply circuits were available. Practically all of the circuits at that time were single-phase, 16,000 alternations per minute (133 cycles). After Mr. Tesla left, Mr. Scott continued the work for a time, developing what was really a pretty fair motor, considering the design handicaps involved in the type selected. This was a polar type machine with the primary coils on individual stator teeth. The merits of the motor, however, could not overcome the handicaps of a lack of a suitable supply system; consequently, the whole matter was dropped.

In 1891, after I had had considerable experience in plotting out magnetic fields set up by distributed windings, as represented by the armature windings of direct current machines and railway motors, I came to the conclusion, in looking over the Tesla motor as built, that one of its greatest difficulties was in the magnetic arrangement of the primary field. My rough figures indicated that a distributed wound primary with coils covering the full pole pitch would give about twice as effective field as the old Tesla type. It appeared too, that the secondary windings, distributed in more slots, would give further improvement. Mr. Scott had already distributed the secondary windings of the former motors, but not nearly to the same extent as, it appeared, would be desirable.

When I talked this matter over with Mr. Scott, he

seemed to think it would be worth while for me to try my hand at such a motor as soon as an opportunity presented itself. Consequently, a short time later, that is, in the latter part of 1891 and the early part of 1892, I went into the matter quite extensively and finally obtained permission to build such a motor. This, I believe was the first induction motor built by the Westinghouse Company which bears any close resemblance to the modern type. Tests on the machine were made by Mr. N. W. Storer, who was then assisting me, and the results were so good that it looked as if we had something well worth following up. At the conclusion of the test, Mr. Storer burnt off the insulation of the rotor end windings and then soldered both ends of his secondary winding solidly together in order to make the equivalent of a cage, which also gave very good results. This, therefore, was the first real cage type motor in our early tests.

By this time, the 60-cycle system was coming in quite rapidly as the standard frequency, but practically everything was single phase, and therefore, there were no available supply circuits for it, even if we did develop an extra good induction motor. At this time, we were getting ready for the World's Fair exhibition at Chicago, and the Westinghouse Company obtained the contract for the lighting of the Fair. It was at Mr. Westinghouse's suggestion that the machines for the lighting plant at Chicago were each made with two single phase generators, side by side, with their armature windings staggered 90°. In this way, each unit furnished two single phase circuits having quarter phase relation to each other. The idea was that this would be a step toward a coming polyphase supply system. These lighting machines were built

in this way, and part of the Westinghouse polyphase exhibit in the Electricity Building at the Fair was operated by means of quarter phase. I cite this to show that even at that time (1892) it was recognized that we should push the polyphase system, in order to furnish a field for the induction motor, and possibly for the rotary converter.

It was in 1892 that we began to consider the use of slotted type armatures instead of the toothed type for alternator work. There was really not much need for the slotted type as long as the single phase machines were used; but as soon as 2-phase or 3-phase, with over-lapping windings, had to be considered, then the need of the distributed slotted construction became more and more apparent. In 1890, I had constructed a slotted armature alternator for some special repair job, thus showing that this construction was feasible.

CHAPTER IV

NIAGARA FALLS—THE WORLD'S FAIR—LORD KELVIN —1892 AND 1893

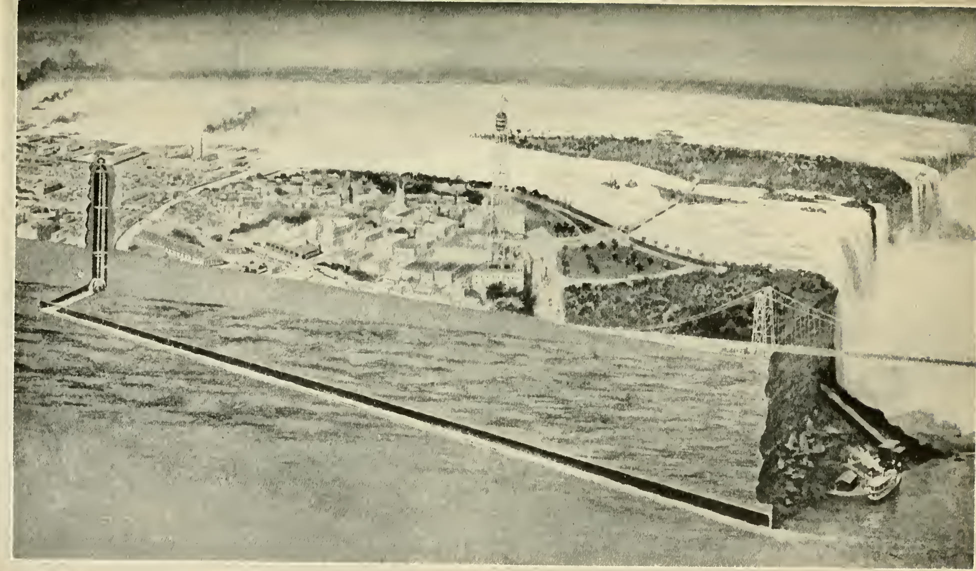
IN 1892, it was decided to begin work on the development of the rotary converter, and, as a preliminary, to make some operating tests. To avoid building a special machine, I took an 850 revolution 4-pole slotted type armature railway generator of about 150 hp. capacity. Over the commutator, four insulated copper rings were placed, with suitable brush holders. This covered about one-half of the commutator face, leaving the other half for direct current brushes. In this way, a rotary was obtained. This was put through a series of experimental tests and very valuable data obtained.

At this time, the Niagara Power development was being seriously considered by a Commission, consisting of Dr. Coleman Sellers, Prof. Rowland, of Johns Hopkins University, and Professor George Forbes, of England, and others. There was considerable discussion among these engineers regarding the frequency to be adopted on the proposed Niagara plant. Prof. Forbes wanted 2000 alternations per minute (16–⅔ cycles) and he had designed an 8-pole machine at 250 revolutions per minute. The Westinghouse engineers wanted a 16-pole machine at 250 revolutions, thus giving 4000 alternations (33–⅓ cycles). Finally, as a compromise, we agreed upon 12

poles at 250 revolutions. Thus originated the 25 cycle frequency.

In the various discussions, the possibilities of induction motors, rotary converters and alternating current commutator motors were gone into to a remarkable extent, considering the slight knowledge of such apparatus at that time. Very serious attempts to foresee the trend of future developments were made. The possibilities of the production of direct current by rotary converters were brought out quite fully. One of the points that came up for discussion was that concerning the copper losses in such machines. Prof. Rowland took the stand that the armature copper losses would be the sum of the alternating current and direct current, that is, it would be much higher than direct current or alternating current alone. Prof. Forbes, who almost invariably took the opposite side from Prof. Rowland, contended that it would be the difference in the losses. Apparently, neither of them had any calculations or data to fall back upon, yet both took a "stiff" stand on this question.

I did not feel like saying much, when such great authorities were discussing the problem, but I immediately arranged to make a running test on the above described rotary which was then set up. We had a very convincing way for comparing the capacity of the machine as a rotary and as a direct current generator. We knew, almost to a dot, how much the machine would carry as a generator for a certain length of time, until it reached the smoking point; so all I had to do was to load the machine up as a rotary to find how much current could be carried to reach the smoking point. The test indicated quite clearly that the current capacity was 40%



Cross Section Drawing Showing Arrangement of Original Niagara Falls Plant. Tailrace is in Foreground.

From *Cassier's Magazine*.

to 50% greater when operating as a rotary than as a direct current generator. In other words, on a machine having a capacity of about 280 amperes as a generator, we carried a little more than 400 amperes. This was pretty conclusive, and when the results were given out shortly afterwards, they were accepted, almost without question, by every one except Prof. Rowland, who would not accept them at all.

In order to verify these tests by calculation, I undertook to work out the losses by a general solution, but found the mathematics rather long and complicated. I, therefore, worked out the losses, point by point, for each 5 degrees of rotation of the armature, and then plotted a loss curve. I did this work practically over night and found that, as a 2-phase rotary, the losses should be 38% of those of a d-c. machine. This I believe, was the first time this problem was ever analyzed to see what the actual loss relations would be.

Mr. R. D. Mershon, who was then with the Westinghouse Company and quite mathematically inclined, also undertook to work out the solution in a highly mathematical manner. It took him about a week to do so; however, his final result checked almost exactly with mine, which was a most pleasing result for both of us. Thus, one of the very interesting characteristics of the rotary converter was determined, once and for all, and this had a very considerable bearing on our future attitude toward this type of machine.

In addition to the above work in 1892, I developed a number of so-called "Kodak" generators, which consisted of direct current generators directly coupled to high speed engines. Some of these were made for rather

low speeds, such as 140 revolutions. All of the generators during this time were made of the slotted type, as it had become the accepted standard.

The latter part of 1892, and the beginning of 1893, and the next two or three years, as far as I was concerned, might be called a period of experimentation rather than new development. The Niagara generators were designed, and developed in detail in 1893 and built the following year. Actual work was begun on a commercial line of induction motors in 1893. Polyphase generators of the slotted type, as distinguished from the toothed type, came during 1893.

In the early part of 1893, much entirely new and novel apparatus was built for our Chicago World's Fair Exhibit. This included a 250 hp. 12-pole induction motor, three rotary converters, one of them of 500 hp. capacity at 550 volts for railway work; also one alternating current-direct current generator of 500 hp. In the operation of this exhibit, the 250 hp. motor received power from the 2-phase lighting machine, described before, and drove, by belt, the alternating current-direct current generator, giving 3600 alternations. From the alternating side of this alternating current-direct current generator, current was supplied to the two rotaries, so that the whole exhibit could be in motion at one time. In addition, one of these rotaries furnished direct current for the operation of some apparatus in another exhibit.

As an example of flexibility in transformations of power, I will mention that, at one time, we wanted to turn down a commutator at one of the exhibits which had been damaged; and in order to do this, we obtained direct current power from one of the rotaries. At the time, I

PART OF WESTINGHOUSE EXHIBIT AT WORLD'S COLUMBIAN EXPOSITION IN CHICAGO IN 1893.

Metal ellipsoid on circular table is the egg of Columbus which stood on end when table was magnetically excited by alternating current.

figured out how many transformations of power were used in turning over this commutator. If I remember correctly, it totaled 13 complete transformations from the steam engines through the generator, transformers, induction motors, alternating current-direct current generator and rotary converter, down to the commutator to be turned. I do not know what was the total efficiency comprising all these transformations, but probably it was no worse than that of the ordinary incandescent lamp of that time.

In the manufacture of this World's Fair exhibit in the Spring of 1893, much of the apparatus was built and tested in the Allegheny Works of the Company. As they were busy in construction on week days, we had to make what few tests we could on Sunday. Mr. W. S. Rugg, then one of my assistants and later Vice President of the Company, with me and one or two others, spent most of his Sundays that Spring in making such tests. It was very tiresome because it was so long continued, as we usually worked from early Sunday morning until late at night. About the only recess we had was when the patrol went by. The Allegheny works were located on the border of what was called the "tough district" of Allegheny; and on Sunday, the patrol wagons went by pretty regularly. Whenever we heard the wagon we ran to the door to see what was happening. We could see crowds collected down the street, but we were too busy to give more than a glance at what was occurring.

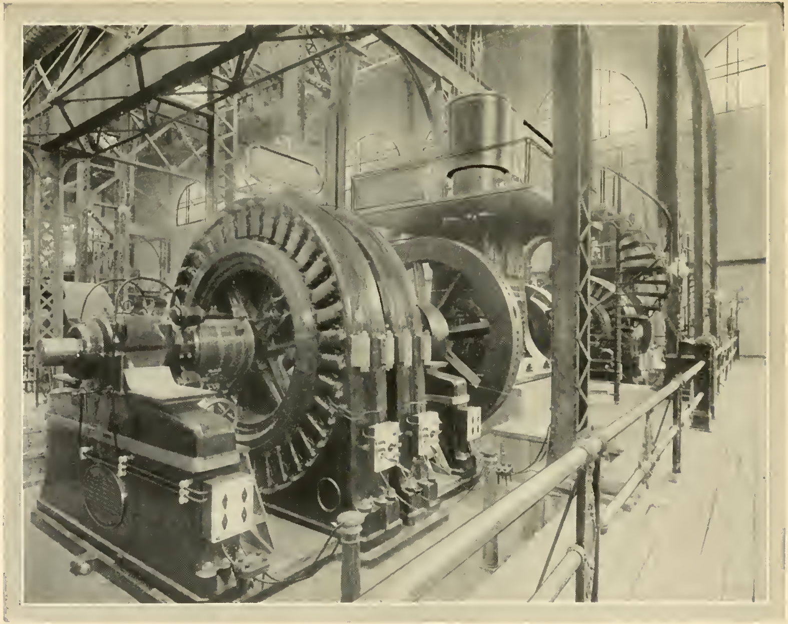
The apparatus being tested was practically all of new types of construction and I was very anxious to obtain sufficient test data to make more advanced calculations. At the same time, the apparatus was long overdue for

the Exposition, so we had to draw a fine line between what we wanted and what we were permitted to do. As a result of the testing, Mr. Rugg was sent to the Exposition to oversee the installation and operation of the special apparatus. This took him temporarily away from the test work, where he had been my assistant for several years.

In 1893, a new line of work began for me, namely the development of a fundamental method of calculation, which, in its more complete form, has been used by the Westinghouse Company ever since. Previous calculations had been to a considerable extent based upon empirical formulæ, which, though good for standard types, were not much good when anything radically new was being undertaken. Back of this development were requests from Mr. Schmid, from time to time, to give him written explanations of various electrical and magnetic actions in our machinery.[1]

In addition to this, from time to time, I prepared short articles explaining, in non-technical language, as far as possible, how our machines actually functioned. I had to explain all such things as the theory of armature windings, the generation of e.m.f. in magnetic circuits and the compounding of machines; and to do this, I had to go into the theory pretty far for those days. The preparation of these papers was of far more help to me than to any one else, as they resulted in my developing much more

[1] As a matter of interest, after Mr. Lamme had prepared several of these papers, Mr. Schmid felt that his own technical knowledge had been so much widened by them that he presented Mr. Lamme with a very handsome watch. Mr. Lamme wore this watch until his death and this tangible souvenir of grateful appreciation no doubt added to the feeling of admiration and kindliness which he always bore Mr. Schmid.



Part of Westinghouse Exhibit at World's Columbian Exposition in Chicago in 1893.

Machines in foreground are two single-phase A. C. generators mechanically coupled to give two-phase current.

complete methods of calculation. Beginning to see wherein many of our existing methods were too incomplete to permit their safe use in radically new work, I set to work, as opportunity offered, to develop a more complete method of analysis.

In this matter of analysis of electrical machinery and development of methods of calculation, I spent many years. Much of this work was done at night; and, counting from 1890 for the next fifteen years I averaged probably three hours per night, five nights per week in such work, developing methods and checking results with practically all apparatus available, both alternating current and direct current, until I felt reasonably sure that I had covered the problem completely. As the method grew, application of it was made to single-phase and polyphase alternators, direct current generators and railway and other motors, both polyphase and single-phase, rotary converters, and practically all other types of machinery, such as would be manufactured by a large electrical company.

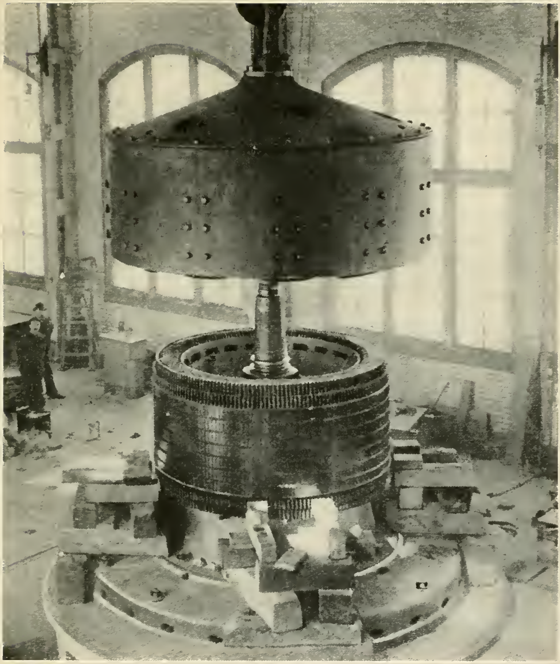
In 1893 and '94, this development of the methods of calculation was given greater impetus by certain work which came up in connection with the first Niagara 5000-horsepower generators. In the study of these machines, one of the engineers made the suggestion that the flux distribution in the air gap could be determined by means of sheet steel templates or punchings, properly magnetized, and acting upon fine iron filings.

Methods of plotting flux distributions by iron filings were not uncommon at that time. Most of them were beautiful to look at, but did not mean much. When the iron-filings picture of one of the poles was shown to

me, I remarked that I believed I could plot the magnetic field more accurately by direct calculation, as I had already done some work of this kind. As a result, I plotted out the flux distribution under the poles, and calculated the e.m.f. of a single conductor, due to this flux distribution. This gave the e.m.f. wave of a single conductor or turn. Then, by superimposing the e.m.f. waves for a complete phase group, I obtained the resultant e.m.f. wave and voltage for one phase. In other words, I reproduced on paper actually what the machine was supposed to do magnetically and electrically.

Later, when the machine came on shop test, I endeavored to check my results by turning the field at an extremely low speed, by means of a cable around its periphery. This machine had an external rotating field of the umbrella type, and, therefore, it was possible to rotate it in this manner. With a direct current voltmeter across its terminals, periodic readings were taken at short intervals, and these were plotted in the form of e.m.f. waves. The e.m.f. curve for a single turn gave the flux distribution or field form. I was much pleased to find that these measured tests agreed almost exactly with my previously calculated results.

I then carried this field form method into other constructions and designs, including alternating current single and polyphase machines, direct current machines, and induction motors, until I had developed a method of calculation that was applicable, in general, to all types of machines; the direct current being simply a special case of the alternating current. It took several years to fully develop this method of calculation, and to apply it to the various types; but it has proved of incalculable



Original Niagara Generator During Installation in Power House.

From *Cassier's Magazine.*

value, especially in the analyses and developments of new types of apparatus. It was gradually simplified and put into mathematical form for easier and quicker application. It proved to be especially valuable in connection with induction motor development. A number of radically new results were obtained by means of it. Broadly, it was an analytical method of design, rather than a purely mathematical one, and the results have been far-reaching. This basic method has never been published by me, although it has been written up. Parts of it have been published at various times by other people.

Development of this method, together with its application to various types of apparatus, covered a period of about six years. Most of this work was done at night, as my daytime was taken up, very largely, with shop work. I made it a practice never to take home any of my regular or routine work, but always to undertake, at night, something out of the usual that was particularly interesting to me. In this way, the evening work was somewhat of a relief from the daily routine.

My faculty for mental computation proved to be of exceedingly great assistance in such work, especially where developments of principles and relationships were of a quantitative nature. By means of this faculty, I believe I was enabled to see relationships not obvious to those who depend upon paper work. For instance, taking a table of apparently more or less disconnected data, I could discover curious relationships and sometimes indications of a rule or a law running throughout. This sometimes opened the way to a new line of study or endeavor. Possibly this faculty for mental computation has been of more value to me than any other major trait.

It has given me a quantitative or dimensional sense, or what might be called a sixth sense.

I have never used a slide-rule, except for a short period in a more or less experimental way. In this short period referred to, I took up its use for a few weeks, some twenty-five years ago, but soon found that I was losing my quantitative sense and my faculty for mental computation. I found also that the figures and results were not retained mentally, as was formerly the case. After going into the matter carefully, I decided I was losing ground by the use of the slide-rule and so abandoned it, and I soon found that my former faculty for mental calculation was acquired again. On account of this experience, I have never again taken up the use of the slide-rule. No doubt for prolonged calculations along routine lines, the slide-rule will beat out mental computation; but, for my own particular class of work, where one is always feeling around trying to arrive at some special result, or is getting at relative results, the mental slide-rule apparently excels the mechanical slide rule. I find that it requires constant practice to maintain the mental method at high efficiency. In my earlier days, when overcrowded for time, many a time, when on a train, or street car, or in bed, or elsewhere, where paper could not be used, I have worked out in my head complete designs for new apparatus. In such cases, very often the work was even carried out in such complete detail that a full working specification, with all necessary data, was written out off-hand the next day without a particle of pencil calculation. The beauty of mental calculation is that the important figures and data are then kept in "mental stock," and can be drawn upon almost instantly when required—

another feature which has been of extreme value in my work.

The hours and days and years spent in mathematical and other analyses, carried on mostly at night, have given me the equivalent of three to five extra working years in my business life; and these have not been hours of wearying routine work, but of something pleasant, because, in many cases, they have represented new and unusual lines. However, due credit for this special work has seldom been given me. For instance, sometimes I have worked out a principle or a problem painstakingly for weeks or even months, in order to get a good mental grasp or physical conception of it; then, possibly months or even years later, some occasion has unexpectedly arisen that involved this very matter. Because I have been able to give, apparently off-hand, a fairly elaborate and accurate opinion, it is frequently said that such things "come easy" to me, or I "don't have to study them out as other people do." It is the same way in mathematical work. That comes hard with me; that is, I have to dig things out in mathematics by long painstaking work. I have persistency enough to stick at things in most cases until I have obtained a result, and my memory is of great help in retaining these results.

My work with the company has been largely in so-called design, although it has been analysis rather than design. In fact, it might be said that my design work is largely analysis applied to practical work. The mere production of a piece of apparatus has not usually been as interesting as the analytical work back of it. In my work, I have always made it a point to see whether a thing looked reasonably possible before putting in any

great efforts. In other words, I figure very closely the chances for success. Often, if it did not look at all promising, I could not arouse myself to great enthusiasm for it, although I could do good sound work on it. Yet, I have often worked on unpromising things and even brought them to full success, contrary to my earlier opinions. In such cases, however, the matters in question did not appear to be fundamentally wrong; usually there was merely a question of whether they could be made practicable or operative. Not infrequently, things have looked unpromising owing to lack of proper data upon which to base an analysis; but in such cases I have always considered that with the proper data available there was a possibility of success.

In taking up the study of a new problem or principle, sometimes I have to turn it over in my mind for weeks before I get a proper perspective. If I once get the right viewpoint, usually the problem is not so difficult.

One would think, from what has already been said, that my time has been taken up, almost entirely, with the study of the principles and practices in electrical apparatus. Such, however, is not the case, as I have had to take up the "man" problem very extensively, almost ever since I came into the Company. The beginning of the present Engineering Department of the Westinghouse Company was my earlier work in the testing room and my following work on methods of calculation. As this work grew, I had to take on assistants of various kinds, many of whom I selected myself from a study of their tastes, aptitudes and characteristics. In my childhood, I think I was a better judge of other people than most

of my associates or schoolmates, possibly because I had a more critical mind.

As I have said before, I knew most of the characteristics of my neighbors. In college I found it necessary for some reasons to study my associates very closely. In my later years in college I think I had my schoolmates pretty well analyzed. This was a matter of evidence by something that occurred many years later. At the 25th Anniversary reunion of our Class, at the school, I met a large number of my oldtime schoolmates and took it upon myself to see how many of them had lived up to my school-day opinions of them. At the time of this reunion, I had had considerable experience in selecting and classifying men for the Westinghouse Company's work and considered myself pretty good at it. It was somewhat to my surprise, however, that I found practically all of my oldtime schoolmates still meeting the specifications that I had mentally formed of them in my school days. The traits that stood out prominently in their school days still stood out, just as prominently; and apparently but few new ones had developed.

For more than thirty years, I have been passing upon men, especially young men directly from school, who are candidates for our engineering work. Under the circumstances, I have had most excellent means for calibrating my opinions, for I have been able to follow these men through their later careers, covering many years. Very few people have had as good an opportunity to check their first opinions in so complete a manner, and naturally this has been of great assistance in later years. I consider that no other person in the Westinghouse Company has had the opportunities and experience that I have en-

joyed in this way. In this selection of men, I have always endeavored to pick those of the highest technical and engineering abilities and to train them to the utmost. People have hinted to me at times that such a procedure must be more or less dangerous to myself, as I would thus, eventually, bring forward men who would take my position. But, I have worked on the doctrine that if the teacher cannot keep ahead of his pupils, he is not a good teacher; and, in fact, I have found that this education and training of the young engineers has been of extreme value to myself, for it has been the means of bringing me into very close contact with all of them in later years, enabling me to keep in closer touch with their ideas, their ways of thinking and the results they obtain. If a man has shown signs of growing rapidly, I have encouraged him to the utmost. In other words, I have not allowed any personal jealousies or feelings to enter into this matter. On account of this general attitude, moreover, I believe the engineers of the Company, as a whole, have shown extreme loyalty toward me in all matters.

I have done considerable technical writing from time to time. As I have mentioned before, in my early years with the Company, I prepared a number of short technical papers for Mr. Schmid, I had had a little practice in writing things up, in general, in high school and college, and had found the study of rhetoric in high school to be extremely interesting, particularly in connection with sentence building, and all matters of English structure. I have found this early study to be of considerable value in later years.

I did not prepare any technical papers for publication until about eight years after I had come into the Westing-

house Company, although I had written a number of technical articles for the use of the engineers before this. I found that technical writing for publication was extremely difficult, not so much owing to a lack of data or ideas, as to the difficulty in explaining things in what I considered to be a sufficiently clear manner. I have written many papers at various times, and I have found all of them to be rather difficult to prepare, no matter what the subject. Usually the first draft is comparatively easy as it contained the general material which is to be included in the article. It is in the "dressing up" of this material that the trouble comes. Usually I have to study each sentence and paragraph of the first draft, in order to see whether the meaning is rightly stated; and after this, I have to review it again from the rhetorical standpoint. In consequence, the paper has to be revised and rewritten several times before it appears to be reasonably satisfactory and I always have a great feeling of relief after it is completed.

One of my great difficulties in early times was in public speaking. In the public schools I was practically a failure at this. If I wrote a thing out completely, I could not remember it word for word, and, therefore, would give only the substance of it when I came to speak, ofttimes giving even this in a very crude manner. When I studied engineering in college, one of the supposed advantages, to my mind, was that an engineer never had to speak in public. In college I did practically no public speaking, as none was required. This was one of the mistakes in my training.

A year or so after I entered the Westinghouse Company, Mr. Schmid asked me to give a series of talks to

the shop foremen on engineering matters. These men were very able from the shop standpoint, but knew little about the theories of electrical engineering and I was supposed to present these matters in a form that was within their grasp. I was to give ten talks, I believe; but by the time I had completed seven or eight of them, I found the task to be practically impossible, as I could not explain some of the matters in a manner which was at all comprehensible to my audience. For instance, one of these talks was on armature windings. Though I did the very best I could, I did not succeed in making them understand the real whys and wherefores of armature construction, especially the surface-wound armature of those days. However, the experience was good training for me, and I did not regret it. During the next few years, the only speaking I did was before some of our salesmen, all of whom I knew personally. This was not very difficult, and I did not dread it.

My first real experience in speaking before outsiders was an extremely trying one, in view of the celebrities in my audience. I had been doing some work on the analysis of the induction motor, from which resulted the development of the Westinghouse type "C" motors, which "took the market by storm." About the time that this development was well under way, I received a message one day that Mr. Westinghouse wanted to see me in a certain room in the offices. I went there and found that Mr. Westinghouse had as a guest, Lord Kelvin, and a number of other celebrities. I thought that he simply wanted me to meet them, especially Lord Kelvin. This surmise was correct; but after the introduction was over, he said that they wanted me to explain to them the fun-

damental principles of the new induction motor, and how I had got at them. In other words, I was put up before the small audience, without previous notice, and told to give a scientific lecture.

The situation was extremely difficult; but fortunately, I had a stout chair to hold on to, so that my limbs did not give way. Mr. Westinghouse, from time to time, interpolated a question designed to bring out some point more fully for Lord Kelvin's benefit. In such cases, I had to explain things directly to this distinguished visitor, making matters still more embarrassing. The only saving feature, from my standpoint, was that Lord Kelvin seemed to be just as embarrassed as I was, and the impression I obtained was that he was an exceedingly modest man. I had always been a great admirer of Lord Kelvin, or Sir William Thompson, as he was formerly known, esteeming him the greatest physicist and scientist at that time in the world; and I had hoped, some day, to have the pleasure of seeing him *at a distance*. However, even in my wildest imagination, I had never thought of getting up and talking before him. It may be easily seen, therefore, that on this occasion, I had good grounds for embarrassment.

With the presentation of various papers before technical societies, especially before the American Institute of Electrical Engineers, I have got more practice in the art of public speaking, but it has never "come easy" for me, and I have never been able to deliver a paper as it was prepared. In other words, the best that I have been able to do is to take the general outline of my subject and stick to it as closely as I could; but in most cases I have even forgotten the outline after once beginning to speak.

CHAPTER V

INDUCTION MOTORS—PIONEERS—EQUALIZER CONNECTIONS—1893 AND 1894

IT was in 1893 that the induction motor began to receive more serious consideration and attention as a commercial possibility. Sixty cycles had meanwhile come in as standard frequency and apparently, from the Niagara development, the indications were that a lower frequency was also coming to the front. Therefore, it appeared to all of us that the time was approaching when the induction motor would be needed. However, the necessity for general supply systems was well recognized, and I believe that it was upon my recommendation that the proper start was made toward remedying this difficulty.

I remember that in a general conference, held on the subject of induction motors, rotary converters and other machines of this type, I suggested that if we should go ahead and fill up the country with polyphase generators, that is, make a fad of the polyphase generation, the induction motor and rotary converter situations would take care of themselves; for, as soon as the supply stations had installed polyphase circuits, they would begin to call for motors, and other polyphase apparatus. This policy was followed and polyphase generators *did* become a fad, and the demand soon *did* arise for induction motors; in fact, the demand came so quickly that we were not ready for it.

Two Views of the Generator Room in the Original Niagara Falls Plant.

Second machine shows external field in rotation.

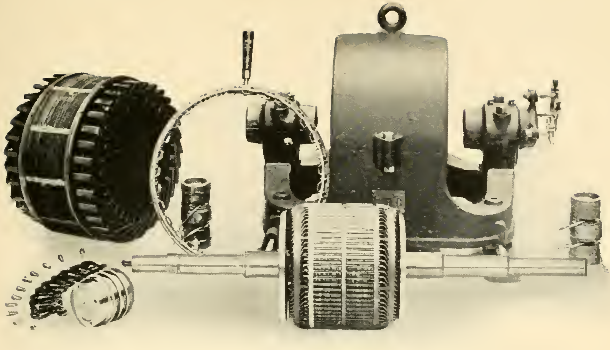
Also, at this time, as soon as we had begun a policy of pushing polyphase generators, it developed that we should have to go into the distributed types of armature windings, with many slots per hole, as this type was the pole only suitable one for polyphase generation. Consequently, the switch-over to this type was quite rapid; in fact, it probably required not more than a year to make the change. As already described, the Niagara generator was laid out with distributed windings. This was a rotating field machine, and one of the first in this country. However, the usual practice was represented by stationary external fields with internal rotating armatures, and this was the standard for several years, except in the case of the Stanley-Kelly inductor type alternator. Also, by this time, 133 cycles was of secondary importance and practically everything had gone to 60 cycles, or lower, although some 66-⅔ cycle polyphase plants were installed.

By 1894, the polyphase systems had become a fad. The slotted type of armature was coming rapidly to the front, rotary converters were being designed for low frequencies, and induction motors were being pushed at a lively rate. The 1893 World's Fair exhibit had shown that rotary converters were practicable machines, and here and there a customer would consider a special generating unit with one or two rotaries. Also, rotaries were being considered in connection with the Niagara plant, which was soon to be started; therefore, I took up actively the rotary converter design during 1894.

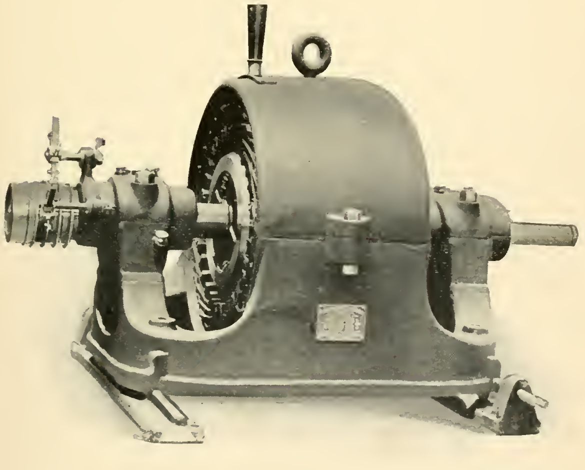
However, possibly my most important development in this year was in polyphase motor work. In 1893, I designed several 25-cycle motors, with distributed prim-

ary and secondary windings, very much like our modern machines, and with partially closed slots in both elements in order to increase the effective cross section of the air gap, and thus decrease the wattless current. These machines, after test, were not considered very satisfactory, as the partially closed slot arrangement did not appeal to our manufacturing departments. Furthermore, as the immediate demand appeared to be for 60-cycle motors, a new line was designed for 60 cycles in which manufacturing conditions rather than refinements in performance were given preference.

In this line of machines, which was afterwards known as our type B, I made the primary the rotating element of the motor, as it was then considered that such motors would not be made for more than 200 or 400 volts. With this arrangement I was able to use a low voltage secondary with leads carried out directly to a starting resistance and short circuiting switch. This appeared to furnish the simplest type of motor with the least experimental development. It was considered that the secondary circuit, with its starting and short-circuiting elements, represented a big problem; and the above arrangement was regarded as the least questionable. Consequently, the line of motors built in 1894 was of this construction; and I will say that the first motors brought through on customers' orders were designed from the ground up by calculation, were shipped as built without modification and even from the present viewpoint were not bad machines. I will say, however, that the 250 hp. machine, exhibited at the World's Fair in 1893, had a rotating primary and a stationary secondary with carbon rod resistance and a heavy short circuiting switch, very similar to the 60-cycle



Parts of Type B Induction Motor.



Type B Induction Motor.

Original form of commercial induction motor with rotating primary.

line of motors just described. Consequently, we had some precedent for this line of small machines.

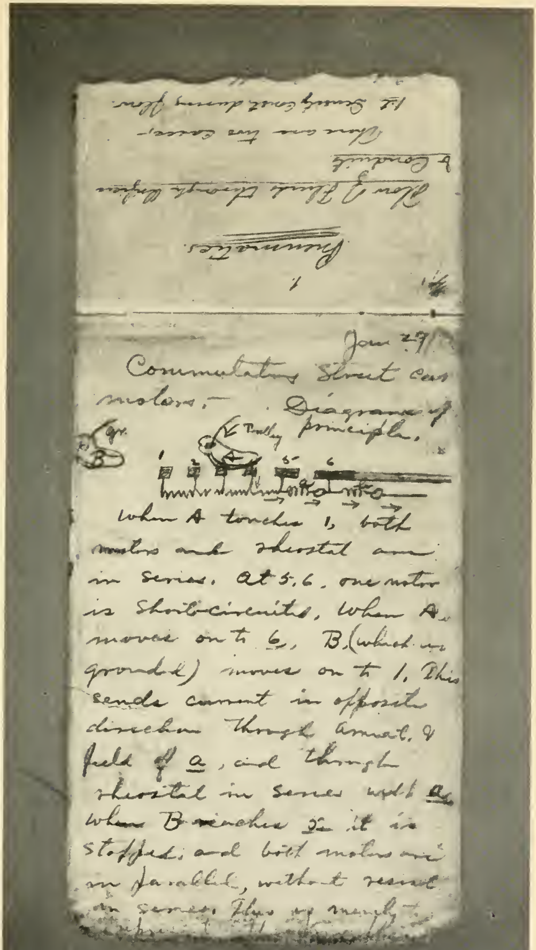
In building and testing this line, of course, a great many problems arose, involving the predetermination of the characteristics of such machines; and I feel that I did pretty well, considering how much was unknown at that time. Moreover, our tests were so arranged that we were continually obtaining new information. For instance, our first motors had secondaries of the closed coil 2-circuit type of winding (like that of a small direct current machine or rotary converter), and four leads were carried off from 90° points, thus giving a two-phase secondary circuit. When at speed, this winding was short-circuited at the four leads.

In one of my tests, having decided to find whether more short circuits would be effective, I tapped onto the winding successively at 6, 8, 12 and more points. The results showed that six taps gave considerably more "pull-out" torque than four, but that the further improvement decreased rapidly up to the twelve taps. Apparently, short-circuiting all around would give only a very small percent better result than twelve taps. However, short-circuiting all around the windings gave possibly 20 percent or 30 percent higher pull-out torque, and considerably better power factor than four taps; so it was considered worth while to take advantage of this. Consequently, I re-arranged the stationary windings so that a rotating ring, placed over the end connections, could be rotated to short-circuit the entire winding when at speed. This was one of my big improvements in the operation of the induction motors, and led later to more careful consideration of the cage armature construction. At that

time I was using every bit of information I could get for improving my methods of calculations and my analysis of the actions of the apparatus.

One might ask how it was possible for me to obtain any time for such work as special analysis and development of methods of calculations, in view of the fact that I had charge of the testing rooms, beside doing much designing on customers' orders, and other work. In fact, for about five years, I was foreman of the test room, although I did not sign myself with that title. During much of this period, I not infrequently spent much time in the evenings at the works, overseeing special tests. In experimental testing on the rotary converter, as already described, I stayed sometimes as late as two or three o'clock in the morning on this work.

As said before, I early adopted the practice, and I consider it a very good one, too, of not taking home with me any routine work. When I did any evening work at home it was entirely on new problems, methods of analysis, study of principles, or something radically different from my day's work, which would, therefore, constitute a change. After I got started on the development of new methods of calculation, practically all of this work was done at home in the evenings. I think I may say that, beginning about 1890 or 1891, for fifteen years I averaged from twelve to fifteen hours a week at such work, mostly at home in the evenings. Moreover, I did not go off by myself, in a "den," but sat down in the living room, filled with people so to speak, and carried on my work amidst the conversation and general chatter. Possibly it is no wonder that I finally found myself in the condition where I could do no studying or calculations on



Commutating Street car motors. — Diagram of principle.

When A touches 1, both motors and rheostat are in series. At 5, 6, one motor is short-circuited. When A moves on to 6, B (which is grounded) moves on to 1. This sends current in opposite direction through armat. & field of a, and through rheostat in series with a. When B reaches 5 it is stopped, and both motors are in parallel, without resist. in series.

A Page from Mr. Lamme's Note-Book in 1896.

Saturday afternoons at the works, when all the people had gone and all noises had ceased. When one gets the habit of working in the midst of noise, complete silence is likely to bother him.

In this night work, I filled many note books with calculations, and the studies of all kinds of new conditions, 99 percent of which were afterwards undecipherable. Apparently, in certain cases I derived short cuts or empirical methods; but some of the steps in the development were not recorded. Consequently, I was never thereafter able to find just how I reached the conclusions I did, although they were apparently much more correct than anything else I could find. Moreover, at later times, I have gone over these note books and found things partially or wholly worked out which I had thought I had begun at a much later period. When one considers that twelve hours a week, for about fifteen years, represents about three years of ordinary working days, it may be seen that I actually put in much time on these special studies.

At this time, I was quite skillful in carrying out calculations in my head. Not infrequently, when I had some radically new design in mind, I would attempt to carry out the calculations after I retired for the night; and in many instances I have worked out entire machines in this way, even down to the ultimate proportions, putting the results and dimensions in writing the next day. It was a bad habit, of course, to work so late at night; but after accustoming myself to this, I could get along for months at a time, or even for years, with an average of about six hours of sleep a night. As I have said, I discovered this in carrying out some of my extra work in school.

All of the above work had been carried on at the Garrison Alley (Pittsburgh) works of the Electric Company and at the Allegheny works, which was a branch of the Pittsburgh shop.

In 1894, the company was building new, very extensive works at East Pittsburgh, and moving began the latter part of that year. On account of clearing up old orders, the test room at Garrison Alley was one of the last places to operate; I stayed with this work until early in 1895.

One of the last tests which I carried out at the Garrison Alley works, was in connection with a 75 Kw. polyphase generator and 75 hp. induction motor which had been sold as a complete plant. In the tests of this outfit, I decided to reverse the conditions and operate the induction motor as a generator and the synchronous generator as a synchronous load, there being no other load on the system. In this way, the induction motor became a generator, excited purely from a synchronous load. This was loaded up to considerably above full capacity, and the characteristics were noted. The induction motor, as a generator, ran at several percent above the frequency of the line, thus showing, on a large scale, that the induction motor could act as an induction generator and that the slip above synchronism was practically the same as the slip below synchronism when acting as a motor carrying a corresponding load. This, of course, was not a new idea, but possibly it was the first time it was carried out on anything more than a laboratory scale. In this test, the induction machine was run at constant speed, and thus it was necessary for the synchronous motor to slow down as the load came on, in order to give the necessary *slip above synchronism* to the induction genera-

tor. It was most interesting to watch this action on a large scale. Later I obtained a patent on the combination of an induction motor as a generator driving a synchronous machine as a load.

In moving to the new works at East Pittsburgh, very considerable modifications were made in the organization of the company, principally in connection with the Engineering arrangement. In the old Garrison Alley works, where practically all of the offices were located, there was no real Engineering Department. Mr. Schmid, who was Shop Superintendent and had charge of the Draughting Room, was not supposed to have any Engineering Department, although it is obvious from the preceding that he had built up one under another name. The Laboratory was supposed to be something in the nature of an Engineering Department, but it really corresponded more nearly to a Research Department of the present time. Consequently, about the only Engineering Department this Company then had was really carried on, fictitiously, as the Testing Department.

In those days, Mr. Schmid's forces and those of the Laboratory were not always any closer to each other than the law allowed. In fact, there was considerable friction at times. Whether there was any real reason for this, I do not pretend to know, as I very seldom was concerned with such matters. I was foreman of the test rooms and was not supposed to keep in close touch with the politics of the situation.

In consequence I never became as well acquainted with some of the "laboratory men" as otherwise I might have. All of this was not the fault, presumably, of either party. Mr. Oliver B. Shallenberger

at that time was Chief Electrician of the Company. People then were electricians and not electrical engineers. As Chief Electrician, he was nominally head of all technical work, and I got to know him quite well, simply through more or less chance meetings in the shop. In fact, at one time, I apparently got into serious trouble with him regarding certain technical matters about which he asked me. I came out in flat opposition to him, so much so that he lost his temper and talked some plain language to me. This looked pretty serious to me, who, as a mere boy in the test room, had got into a row with the chief technical authority of the company. I explained the situation to Mr. Schmid who simply laughed about it; but that did not altogether relieve my mind, for I rather expected to be penalized in some way, even up to the extent of being dismissed. However, somewhat to my surprise, Mr. Shallenberger always took my part, thereafter, in discussions where there was any considerable disagreement; and in some cases, where I stood practically alone regarding certain new or proposed practices, he was sometimes my only "backer." This of course, gave me larger ideas of the man himself; and I have always looked back with the greatest pleasure to my acquaintanceship with such a man. His health was failing, and I saw less and less of him after 1894, as in the later years of his life, he seldom came to the works.

Among other laboratory men, with whom I had some contact, were Mr. L. B. Stillwell, who was Assistant Electrician in the early years, and Electrical Engineer later; and C. F. Scott, whom I had known in school, and with whom I was in very close touch in some of my earlier years with the company. Mr. Scott handled the induc-

tion motor work with Mr. Tesla in 1889 and 1890, much of the work being done in one of our testing rooms. Although I had nothing to do with the work at the time, I saw Mr. Scott so often that I could keep in touch with both him and his work. Also, there was R. D. Mershon, who did much work on induction motors and various other experimental apparatus. Mr. Mershon and I had been fairly closely associated at school; and, although we were, more or less, in different departments in the Westinghouse Company, we still kept close to each other. There were also, Arthur Hartwell, who later became Sales Manager of the company; A. S. Morris, Alex. Wurts, of lightning arrester fame, and a number of others.

I became acquainted with Mr. Wurts very early, through his always "blowing up" his arresters in the test room. My interest was more in the possible damage he might do to the testing machinery than in the action of the arresters themselves. One of my first remembrances of Mr. Wurts goes back to the spring of 1889, during my earliest apprenticeship in the test room of the company, when I was ordered to sit at the throttle of one of the main engines to shut it quickly, in case anything broke, while Mr. Wurts was "blowing" some special fuses. Those old-time surface wound alternators, with almost no reactance, did certainly give a terrific current in case of a dead short; and there was grave danger of wrecking something. From this time on, I knew Mr. Wurts pretty well and had much to do, in later years, in furnishing apparatus and test conditions for the numerous tests which he carried on in his lightning arrester work.

In the test room, in those early years, I had a number

of associates and assistants at different times, one of the earliest of these being N. W. Storer. Others were: W. S. Rugg, later head of our Sales Department; W. F. Lamme, my brother; Norman McPherson, who assisted me in early calculations; C. E. Skinner, now Assistant Director of Engineering, with whom I have been closely connected since the earliest work; R. S. Feicht, who began in the test room, later went into the construction department, still later became one of the calculating engineers of the Company and is now Director of Engineering. Messrs. Feicht and Skinner came with the company in 1890.

In 1891, Mr. H. P. Davis, now Vice-President of the Company, entered our Garrison Alley test room. After a short time, he went into the "Detail" Department as an engineer with Mr. Lange, then Superintendent of the Detail Department. He made a specialty of this work for many years; and, upon our removal to East Pittsburgh, he became head of the Detail Engineering Department.

Philip Lange, when I came into the Company in 1889, was the head of the Detail Department which built instruments and various other apparatus of that general nature, such as the company required in the electrical business. I became acquainted with him very early, was closely associated with him through all the years he was with the American Company, and was especially close to him after we moved to East Pittsburgh. As said above, Mr. Davis very early went with Mr. Lange in his work. Also, associated with him in his work was Mr. Frank Conrad, who came to the company when a boy, and with whom I have been on very close terms ever since.

Like Mr. Schmid, Mr. Lange did not have any real Engineering Department, but built up the equivalent of one under another name.

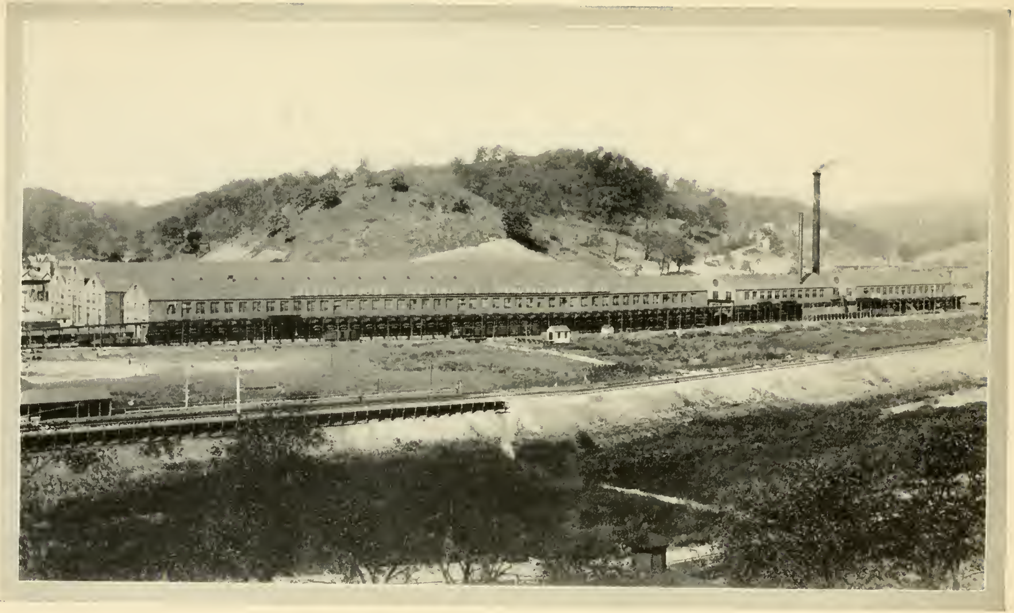
There were, also, in the test room in those times, a number of well known people, not now with the organization; E. R. Hill, of the firm of Gibbs & Hill, W. E. McCoy, of the United Electric Light and Power Company of New York, and numerous others. Among the later men whom I recommended for our test room in the Garrison Alley plant, was P. M. Lincoln, who came to us from the Brush Company of Cleveland. Miss Bertha Lamme, my sister, entered the employ of the company at Mr. Schmid's request in the latter part of this period. She had taken an Engineering degree in the Ohio State University, more for the pleasure of it than anything else; but sometime, later, Mr. Schmid gave her a serious invitation to enter the employ of the company, and so she took up the work of calculation of machines and stayed until she married.

In building up the organization at East Pittsburgh, in the early part of the spring of 1895, many changes were made. The Laboratory was abandoned and the Engineering Department was formed, with Mr. Albert Schmid as head. The transformer work, which had formerly been handled at the Garrison Alley Laboratory, was transferred to this Engineering Department under Mr. C. F. Scott's charge. Mr. Philip Lange was made Superintendent of Shops. I also was transferred to the new Engineering Department. This ended my direct connection with the Testing Department. After a little time the work ran on as smoothly as ever, and very much along the same lines as before. From this time on, it became part of

my business to help the Engineering Department by selecting suitable new men from time to time. Mr. Storer was my direct assistant at this time, as he had been at Garrison Alley.

During this period, some very large problems were becoming of grave importance. For instance, parallel operation of alternators had been of relatively little importance in the old days of single-phase lighting generators. Moreover, practically all of the early machines had been belted, which made parallel operation much easier, although this was not appreciated fully at the time. However, when the polyphase generators began to come into general use, the engine type alternator also was coming strongly to the front. This latter made the paralleling condition much more difficult just at the time when the need for paralleling, due to supply of power circuits, was increasing. Consequently, we had to promise parallel operation in many cases and take our chances on obtaining this result.

For instance, in laying out the plant for our new East Pittsburgh works in 1894, 25-cycle polyphase generators were selected for the power-house equipment, with induction motors throughout the shop for general power purposes. The plans absolutely required parallel operation. I remember, that, as the plant was approaching completion, Mr. Lange, who had then been made Shop Superintendent, suddenly discovered that the successful operation of the whole plant was contingent upon parallel operation of the generating units, which, to some extent, was in the opinion of most people in an experimental condition. He was much excited about it and asked me what we expected to do about it. I told him that the



THE EAST PITTSBURGH PLANT IN 1894.
Just after the move from Garrison Alley.

machines just *had* to run in parallel, and that, if they did not, we would just *have to make them do so.* However, as the alternators were coupled direct to their engines through a method of spring drive, and as we had already indications that some flexibility in the drive was an important item in the operation, I had little fear for the result. Shortly afterwards when they started up the plant, they threw the machines together without even a whimper. Their operation was successful for many years, until finally superseded by turbo-generators.

During the following three or four years, parallel operation of engine type generators was a very serious problem with all of the manufacturing companies. It was recognized that suitable flywheel capacity was an important part of the problem; also the sensitiveness of the engine governor had much to do with the matter. Especially after rotary converters came in, the smooth running of the engine type alternators became a very essential condition. In some cases, the engine rotation was so irregular, that is, periodic variations were so unduly large, that two generators would not operate in parallel with each other; and yet rotaries were expected to operate successfully on such a generating plant. I will refer to this matter again in connection with the development of dampers on rotaries and generators.

One serious trouble had developed during 1894, in connection with "parallel" windings on large engine-type generators. In the development of small railway engine-type generators in particular, the 2-circuit type winding had been carried up to about 400 or 500 kw.; but, obviously, it had reached its limit of good operating conditions. Consequently, the next step was to the parallel

type of winding, especially as the engine type generators of 800 to 1200 kw. and even 1500 kw., were coming in. In these machines, evidence soon developed that the magnetic circuits of the different poles of the generators were likely to be of unequal strengths, so that various armature circuits in parallel would have unequal e.m.f.'s generated in them during rotation. Apparently, as long as the unbalance was very small, this did not have any serious effects, other than loading the armature circuits unequally and affecting the commutation to some extent. However, when one pole, for instance, was so weak, compared with others, that its armature circuit not only did not take its proper share of current, but even received heavy current from the other circuits, then the conditions might become extremely bad. The explanation was obvious to me as soon as I encountered this phenomenon; for, some two years before, during my work an analysis of field distortion in connection with the compounding of generators, I had discovered that the effect of cross magnetization in a machine with highly saturated armature teeth was to *reduce the total flux per pole.* In other words, the effect of distortion was to weaken one edge more than it strengthened the other. Consequently, in such a machine, regardless of whether it was acting as a motor or as a generator, the armature cross ampere turns tended to weaken the main field flux. I tried this on a large machine about 1892, by running it as a motor, with fairly heavy load, to see whether, with highly saturated teeth, the speed of the machine would *rise* with increase in load. It actually did so, which confirmed my analysis.

When I first encountered the serious unbalancing of

the circuits in large parallel wound generators, there was evidence of enormous local currents under certain conditions. These were so great that in a very short time the brushes got red hot in their boxes. I made tests and discovered that the magnetic circuits, when the brush holders were all disconnected from each other, were so unequal that in some cases there was as much difference as 8 percent or 10 percent in voltage with the probability of only one or two poles showing such low values.

What was happening was then obvious to me. Enough current would be *fed back* into the weak circuits to produce excessive distortion, with consequent further weakening of the weak circuits, and thus the machine would become cumulatively worse until it was necessary to shut it down. The trouble was thus well understood, but the remedy was not directly forthcoming. We tried various "stunts," such as putting balancing resistances in the armature circuits and adjusting the field strength of the different poles. We managed to keep our machines going, but I was not at all satisfied. We presumed that some of the other companies must have had some of this trouble, too, although the General Electric troubles were apparently not nearly so serious as our own.

At that time, the General Electric used solid steel poles *bolted* to the yoke, whereas, in the Westinghouse machines, the poles were of laminated steel *cast into* the yoke. This latter construction, owing to possibly bad flaws in casting, naturally tended toward greater inequalities than were in the bolted-in type of pole. Furthermore, the bolted-in poles allowed the possibility of compensating for inequalities in total flux per pole by means of adjustable air gaps. Whether the General Electric

did follow such practice or not, I do not know, but apparently, as has been mentioned before, that Company had much less trouble than we had. Possibly it was a fortunate thing that we did have serious trouble, as it led to a solution of the difficulty, which is now universal practice.

The trouble came to a head in the following manner: In our testing room we had on test a 14-pole, low voltage, heavy current generator built for electro-deposition work. This had a parallel wound armature. On test it showed much vibration and excessive sparking. Mr. Storer and I worked for a long time trying to balance the circuits in this machine, and Mr. Lange put in his best efforts in trying to have the machine lined up with the greatest accuracy. We could get the machine running properly, with good commutation and no vibration, but if we shut it down and then started it up again, without touching anything about it, it was liable to "go bad" again. This was the first machine we had struck which was too bad to ship. Consequently, we were all much worried. Among our various tests on unbalancing of parallel wound machines, it had been noted that when the brushes were given no-lead, the unbalance was worse than with a forward lead; and that with a slight *back* lead, it was still worse.

At this time it so happened that we had in our test room an alternating current-direct current railway generator which we had built for a certain customer; this was, however, not yet needed. This machine had eight poles, and a parallel wound armature, and we were using it for driving other machinery. While we were discussing our unbalancing troubles on the machine just described, it

suddenly occurred to me that the alternating current-direct current machine with its parallel winding, did not show any evidence of unbalancing, under any conditions of operation. I called attention to this and intimated that I believed the alternating current connections on the machine had something to do with it. That was all that was needed and I immediately worked out the explanation of the true action.

Mr. Lange then suggested that we check up my theory by putting a set of temporary three-phase connections on the 14-pole machine which had been giving trouble. These were placed on the winding immediately over the commutator necks. The work was finished about midnight that same night, and special testing men stayed to make tests. On starting up the machine, there was absolutely no sparking and no vibration. Mr. Lange was so notified by telephone and he immediately authorized them to cut off the cross connections, but not to touch the machine in any other way, and then repeat tests. When this was done, the machine sparked and shook as badly as in the first place. This was considered ample proof of the effectiveness of the device; and we were all quite happy, as we felt that we had found a fundamental solution of the trouble, by the use of equalizer connections on parallel wound d.c. machines.

An interesting incident in connection with this same generator was that it was discovered upon its installation on the customer's premises, that there was difficulty in holding its voltage up to full value, although there had been no such trouble in our shop tests. Mr. Storer was sent out to investigate the trouble and found, first of all, that two of the field coils, namely No. 6 and No. 9, had

been interchanged, (possibly owing to reading the numbers upside down). This had reversed the polarity of two of the poles. In spite of this reversal, the equalizer connections had established the proper polarity under these two fields. Nothing could have given us a stronger proof of the capabilities of these cross connections.

The development of the equalizer connections proved to be a big step in advance in direct current machine construction. We immediately added them to a number of machines already installed, usually putting on three sets of connectors, corresponding to a 3-phase winding. In one of the early cases, we still found evidence of unbalance and then tried six cross connections. This improved matters, and we then tried nine and then twelve. With each increase, we found some improvement, but it was more gradual; so that with twelve cross-connections on thirty-six commutator-bars-per-pole, apparently we got about all that was to be gained in the use of cross connections. Later practice has tended toward the use of a large number of equalizing connections. This development of equalizing connections took place in 1895 and 1896.

CHAPTER VI

INDUCTION MOTORS—ROTARY CONVERTERS—ALTERNATING CURRENT GENERATORS—1895 TO 1900

IN 1895, there arose a demand for alternating current motors for use in hoists, crane work and the like. Especially in connection with crane work, it appeared to me that the wound-secondary type of motor was too complicated, as it required doubling of the number of trolley wires on the crane. It occurred to me, therefore, that if an induction motor of permanently high resistance secondary were used for this work and the applied voltage varied, the speed could be varied up and down at will. In order to check this, a motor was equipped with a special high resistance secondary and tested under a hoisting load. It was found that the speed could be varied very nicely in this manner. I then set out to develop what proved to be a revolutionary improvement in motor practice.

Preliminary to this, I will say that in my studies and analysis of the induction motor, during the preceding year or so, I had been much interested in Prof. Kapp's vector diagrams in his fourth edition of *Electric Transmission of Energy*. I undertook to apply his diagrams to several of the motors which I had built, and found that, when I could determine the reactance with sufficient accuracy, I could plot the entire speed torque curve pretty closely. I then investigated the effects of varying the secondary resistance and reactance in connection with

the experimental hoist motor which I had been working on, and the diagrams indicated pretty clearly the relations of primary and secondary reactance and resistance, to give any shape of speed torque curve that I wanted. This, of course, was nothing new or radical. However, I went even further to find more accurate methods of predetermining the reactance of the motor, including the end windings; and, in doing so, I discovered the fact that by suitable proportioning, I could make the reactance so small that the motor with the cage type secondary winding, could be given a "pull-out" of several times its *rated full-load torque.* The results were so interesting that I developed them into a set of mathematical equations, derived directly from the vector diagrams.

With these equations at hand, I then started in to vary the relations of reactance and resistance with cage-wound secondaries, to see whether constant speed motors could be made with sufficiently high starting torque. At that time it was very generally believed that a motor with a cage secondary must necessarily have a very low starting torque, unless it had a very high slip at rated load. However, my analysis showed that I could get as high as twice full-load torque at start with a full load slip of less than 5%. Under this condition, however, my calculations showed the primary current to be quite large, while if I made the starting torque smaller, the primary current would be still larger. So I decided to reduce the starting torque *by lowering the supply voltage at start, and thus very greatly reduce the line current.*

Thus was developed the Westinghouse type "C" line of induction motors. The first application of these motors was to crane work in the Westinghouse Machine Com-

pany's shops. These first motors gave three to four times full load starting torque with full voltage applied. However, they were next laid out with a view to using them for constant speed work, and as soon thereafter as possible (in 1896) they were ready for the market.

There was much "cold water" thrown on this type "C" motor at the start, even by some people in the Westinghouse organization. It was contended by many that the entire principle was wrong, as a cage wound secondary *could not* give high starting torque. Also, the fact that we used auto-transformers for *reducing the supply voltage*, thus *further lowering the torque*, was adding insult to injury. In some cases, in the commercial field, when certain people wanted to refer to a piece of apparatus as being unusually bad, they said it was "as bad as the Westinghouse type 'C' motor."

Notwithstanding all this, Mr. Westinghouse believed in the motor and its possibilities and urged that it be pushed commercially. This was done; and it soon gained headway and took so well, especially in industrial establishments, that this general type soon held supremacy over every other kind. Later it captured the whole world, and it maintains its position even to this day. The type "C" motor itself lasted some ten years commercially and then went "down and out" in competition with cheaper machines; but all of the later machines have contained the principles involved in this first type. In 1897, I prepared a paper on the subject of "The Polyphase Motor," which explained the principle of the type "C" machine.[1] This little pamphlet is still used, to a

[1] Presented before the Convention of the National Electric Light Association held that year at Niagara Falls.

considerable extent, for technical and educational purposes.

Also, in 1895 and 1896, the rotary converter began to attract much attention. The earlier ones, however, were always for low frequency, such as 25 or 30 cycles. In 1896, many inquiries were received regarding rotaries for 60-cycle circuits. At first we kept away from this; but in 1897, we took a contract for a 250 Kw. 60-cycle rotary.

However, even in 1895 and 1896, we were beginning to have our troubles with rotaries from a new and unexpected source, namely, "hunting," which had developed in isolated instances. I believe we first encountered this on a rotary to be used as an exciter, built for the Niagara Power Company in 1894. Hunting developed once or twice in shop test of this exciter, but apparently it was only at rare intervals and was not considered at all serious. However, during the operation of this exciter at Niagara, a year or so later, real trouble, due to "hunting," did develop. In fact, it was so serious that we could not operate this machine satisfactorily, though at this time, in 1896, we did not have enough rotaries in operation really to appreciate how serious the trouble might be.

It was in 1897, that we "got in deep." We had sold a number of rotaries for electro-chemical plants at Niagara Falls and, upon attempting to operate them, we found that they all "hunted" badly. I spent about two weeks at the Niagara plant, investigating the trouble and looking for a remedy. The cause of the trouble and the remedy were finally determined from a little investigation, from which we did not expect much at the time. Mr. P. H. Knight, one of our electrical engineers, and I

decided to explore the flux distribution in one of the worst machines. We first ran the machine at constant speed from a separate source, and, by means of a templet around the commutator and two voltmeter leads, we measured the voltage in steps all around the commutator. We were thus able to plot a field form for the rotary. This test gave a nice, symmetrical curve, as I had expected. We then operated the machine as a hunting rotary, at no load, and also measured the voltage around the commutator in the same way as before. It was observed at once that the voltage between two adjacent points in the commutator would rise and fall in time with the "hunting" of the machine. We measured the extremes, up and down, for each set of points and then plotted the results. This showed two sets of field forms, one distorted in one direction and the other in the opposite. In other words, the armature reaction during "hunting" was alternately distorting the field in one direction and the other. This of course was not altogether unexpected, but it gave us a measure of the effect and this enabled me to do some calculating in regard to the possibility of "damping" out such effects.

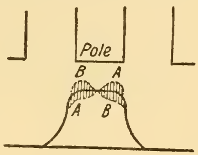
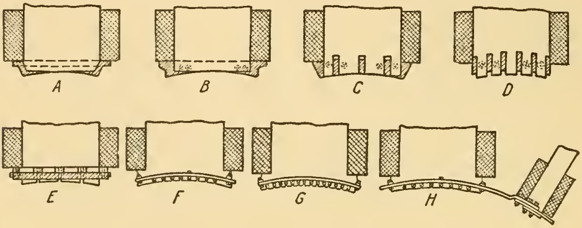


Diagram showing the effect of hunting on the magnetic field of a rotary converter

The machine in question had a very heavy bevel at its pole corners. I figured out that I could put in a heavy enough copper plate over the bevel, and between the poles, to suppress pretty nearly the periodical distortion of the field. This looked interesting. In addition, I had found that a short circuit around the field poles steadied the

field excitation and was thus helpful, also. I, therefore, arranged to apply a set of "copper dampers" surrounding the field poles, with heavy *lips* projecting under the pole corners. This was tried out and was found to be very effective indeed; and, from this time on, copper dampers were used on rotary converters.

However, during this period of investigation, we had to do something to keep our machines going, and it was discovered that if the rotaries were operated without any

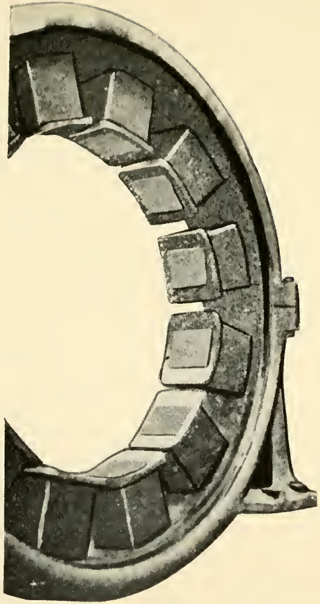


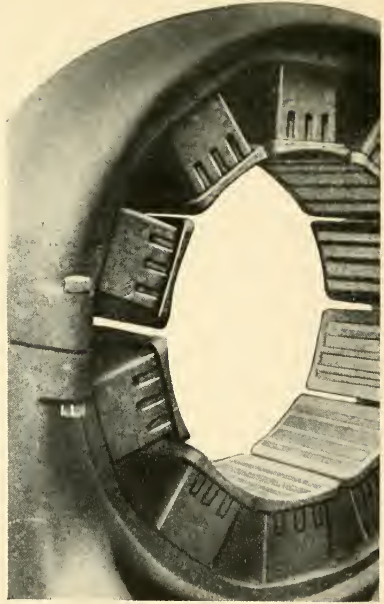
Damper Windings

Showing the development from the earliest type to present day construction

field charge, that is, with excitation from the armature only, they were much more stable. As this looked like a promising idea, we started out to develop what we call "induction rotaries"; but before we got very far with this work, the damper construction came along, proving so much better, that the induction rotary was dropped.

An interesting incident in connection with the use of dampers came up during the discussion of rotary converters at one of the A.I.E.E. meetings. Some of our good friends in the Engineering Department of the General Electric Company claimed that the hunting in the Westinghouse machines was due to the use of 2-phase instead





Solid Cast Damper Grids for Field Poles of Rotary Converters.

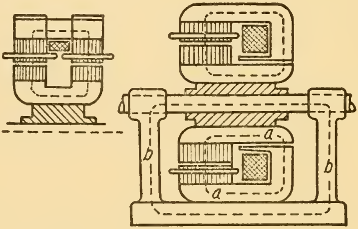
of 3-phase machines, and that 3-phase did not hunt. However, they were due for an awakening, as some of them said afterwards; for, after they got into the matter pretty well, they discovered that 3-phase converters would hunt as well as 2-phase; and, therefore, they also had to develop dampers. At first they used solid poles in their rotaries, which, in themselves, gave some damping action. Later, in going to laminated poles, they found that dampers were necessary. This use of dampers on converters was originated by me; but when applying for broad patents it were discovered that M. Leblanc, the well-known French engineer, had already covered their use in connection with alternating current generators. A license under the Leblanc patent was then obtained, as a matter of protection to the Westinghouse Company.

It soon developed that 60-cycle rotaries needed dampers just as much as 25 cycles, or possibly even more so; but, the difficulty here was to apply the dampers, as the air gaps of such machines were necessarily small. We did it at first by "brute force" at the expense of considerable eddy loss; but we made the machines work.

In 1896, the Westinghouse Company took up the construction of a line of "inductor type" alternators. The Stanley-Kelly inductor alternator having, apparently, been making a favorable impression on our commercial departments, it was claimed that we must have a line of such machines to meet competition. It was stated, particularly, that the inductor alternator was especially good for handling inductive loads. I did not believe this, and later evidence confirmed my opinion.

When the matter came up, (in 1896), with regard to con-

structing such a line, I made a written report in which I said, definitely, that I thought it was a mistake to undertake a line of inductor alternators. I stated further, that a rotating field type of machine, with the internal pole type of field and an external armature, *i.e.*, our normal type of machine turned inside out, possessed all of the real advantages of the inductor alternator, and was materially better as to weight and cost. I stated that from the theoretical, as well as the practical standpoint, it was



Magnetic Circuit of Inductor Alternator

my opinion that the inductor type would go "down and out" before a well designed rotating field type of machine; and that, therefore, we should undertake the development of the latter. Our commercial departments at that time insisted, however, that, regardless of the theoretical merits of the case, they had to have the inductor type. Therefore, it was chosen. Incidentally, the General Manager of the company at that time told me privately that he strongly suspected that I was right in my stand.

We were thus delayed about two years in taking up the construction of the rotating field type and in this development were antedated by the General Electric Company which brought out the rotating field type to meet

the inductor type competition about the same time that we put our inductor type on the market. It may be interesting to note that only four inductor alternators were built before we "switched over" to the rotating field type. The inductor type competition died down so quickly that our real competition was the rotating field type. My records show that in January, 1898, design specifications were put in by me covering a rotating field 850 Kw., engine type alternator. It is thus evident that the switch-over was accomplished very quickly. Of course, in our Niagara machines we used the rotating field construction, but that was not the type that has been adopted as standard in later times.

The year 1896 also marks the beginning of our turbo-generator work. The steam turbine was for many years one of Mr. Westinghouse's "pets" at the old Garrison Alley works. He almost always had some kind of a steam turbine running on test. However, as the Parsons turbine, in England, was attracting much attention, about 1895 and 1896, arrangements were made by which the Parsons Company was to furnish us with a steam turbine for which we were to build a direct current generator. One of the interesting things about this first work was that we undertook to build a direct current machine for conditions much more difficult than anything we have undertaken since. We tried to build a 120 Kw. generator for 5000 rpm. which, after our later direct current experience, would have been considered prohibitive; but, in our ignorance of the possibilities, or rather the impossibilities, we went ahead. We built the machine for about 9000 ft. per minute commutator speed, and used copper brushes. After many disagreeable experi-

ences, such as a broken shaft, and other minor accidents, the machine was placed in service and ran for a while, though as might have been expected, it did not "stay put."

While this was not my own particular work, I wish to bring it in as a record of the beginning of turbo-generator work in the Westinghouse Company. The real turbo work began, apparently, in 1898, in the design and construction of several 300 Kw. turbo-alternators of the rotating armature type, which were installed in the power plant of the Westinghouse Air Brake Company. Those were 3600 revolution per minute, 2-pole machines. They were kept in service for about two or three years. We obtained much valuable experience from these machines, especially with regard to what *could not be done;* this proved quite useful to us in later work. To show how the turbo generator sentiment was growing, I find that in December, 1898, I prepared specifications for a 1500 Kw. rotating field, 6-pole, 1200 revolution turbo-alternator, which was afterwards built and installed in one of our customers' plants.[1] Thus we started on a pretty big scale very early in the "game."

One other new development came up in about 1897; namely, the construction of a number of single-phase, 60-cycle, series wound, commutator-type motors for variable speed work, in one of the Swift Company's packing house plants. Motors of from 5 to 40 horsepower were built, and were operated for from six months to a year. The reason why this is mentioned here is that these machines were really the predecessors of the single-phase

[1] Hartford Electric Light Company. The first large turbine in a central station in America and at that time the largest in the world.

Early Form of Stator Field for Turbo-Generator.

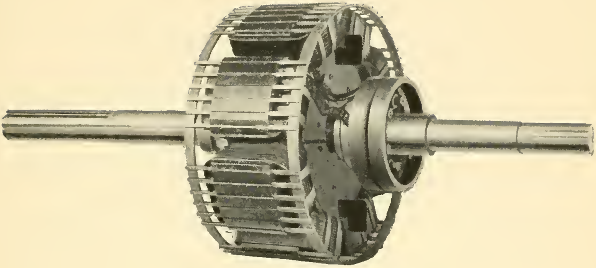
commutator-type railway motors, brought out some three or four years later. These 60-cycle motors met the conditions of service for which they were built; but they required much maintenance in the way of commutator and brush attention, with corresponding expense. After several months of operation, experience showed that practically as good results could be obtained with ordinary induction motors with suitable gearing; and, therefore, these motors were gradually replaced.

With the development of the damper for lessening hunting on rotary converters, obviously, it would follow that we would try these on alternating current generators where hunting developed. This was done with very appreciably improved results, the improvement being so marked that later practically all of this company's standard rotating field, engine type alternators were equipped with dampers of some form. This so helped matters that paralleling of alternators thereafter presented almost no trouble, and the operation of rotaries became more satisfactory. But it was soon found that 60-cycle rotaries were not nearly so satisfactory as 25-cycle, for several reasons. In the first place, it was not possible, with the designs then available, to add sufficient damper to the 60-cycle rotary poles; also, the 60-cycle engine type generators had relatively bigger angular variations in electrical degrees than in the case of 25 cycles. Then again, it was not practicable to put so heavy dampers on the 60-cycle generators. Consequently, from the first, 60-cycle rotaries were looked upon with suspicion, whereas, in many cases, the troubles were not all in the machines, themselves. I spent many years, off and on, working at this problem, at one time standing nearly alone in my

belief that the 60-cycle rotary would some day come into its own. It may be said here that the coming of the turbo-generator vastly helped the situation; and with better dampers and other improvements in construction, which will be described later, the 60-cycle rotary has now come to the front and is holding its own.

It was sometime during this period that I developed the 3-wire rotary converter. Some of our people had been working on rotary converters, measuring the voltage relations between one of the a.c. collector rings and one of the d.c. brush holders. This gave a pulsating e.m.f., varying from zero to a maximum, equivalent to direct current e.m.f., with an alternating current e.m.f. superimposed, the peak value of the alternating current wave being equal to the direct current e.m.f. It was thought at that time, that something might be done with such pulsating e.m.f. having a direct current component.

In reviewing this matter one day, trying to sketch out the e.m.f. conditions, assuming the alternating current connections were made at some distance in from the transformer terminal or collector ring, I obtained an e.m.f. curve, consisting of a direct current e.m.f. with a smaller alternating current superimposed; in other words, the minimum did not reach a zero value. Moving still further in, it appeared that the alternating component of the resultant e.m.f. became smaller and smaller until at the center of the transformer (two-phase being considered) the alternating current component disappeared entirely, what was left being a mid-way point between the two direct current terminals. As this, of course, appeared to be a scheme of important value, I immedi-



Cage Type Damper Winding.

Applied to rotor of synchronous motor.

Early Form of Rotating Armature for Turbo-Generator.

ately had tests made to determine whether it was entirely practicable.

A little time afterwards, Mr. Calvert Townley, then head of our New England Sales Office, was carrying on a negotiation with the Hartford Electric Light Company for some 3-wire direct current apparatus to be used in connection with their 60-cycle supply system. Mr. Townley was notified that we could furnish a single rotary converter with one commutator and no auxiliary appliances of any sort, with a special connection, however, by which we could give the desired 3-wire circuit. Without knowing how the result could be obtained, Mr. Townley immediately offered such an equipment to the customer. The scheme appeared to be so radically new and different from anything else promised, that the sale was consummated at once. Mr. Dunham, then President of the Hartford Electric Light Company, had a reputation for wanting to be first in the field to use something which appeared to be radically new and represented future practice. Consequently, this situation exactly fitted his attitude. The type of equipment was accepted both by Mr. Townley and the customer's engineers, purely on faith that the engineers of the Westinghouse Company could live up to their promises. Shortly afterward, Mr. Townley asked me personally to explain the principle to him, which I did, telling him what tests had been made. This was the beginning of the 3-wire rotary business in America. In this case, unlike many others which I have cited, the scheme was worked out before there had been any real call for it; it was not a remedy for some existing difficulty.

It was also sometime during this period that the later

well-known scheme for operating inverted rotaries at practically constant speed, with variable reactive load, was developed. The first evidence that inverted rotaries were likely to run away with inductive load appeared in one of our shop tests, owing to an accident. One of the testing men, apparently by mistake, threw a heavy inductive load on the alternating current side of an inverted rotary. Before anyone could prevent it, the machine ran away and "exploded." After an investigation, Mr. Lange was inclined to blame the accident on a mistake in operation, the true cause not being suspected at the time.

Some months later, however, we installed a fairly large rotary in our East Pittsburgh power plant, in order to tie together our alternating and direct current sections of the plant, so that either could act as a reserve for the other. The first Saturday after this rotary was installed, it so happened that a number of the shop sections were kept running during the afternoon with practically no load. The induction motors throughout the shop were thus operating at very low-power factors. The power house operator noticing this condition of small power load, assumed that as the total power was very small, he could just as well switch everything to the alternating current side of the rotary and, therefore, shut down the alternating current plant. He was hardly able to make the "switch-over," before the rotary speeded up so rapidly that its end windings burst and wrecked the machine.

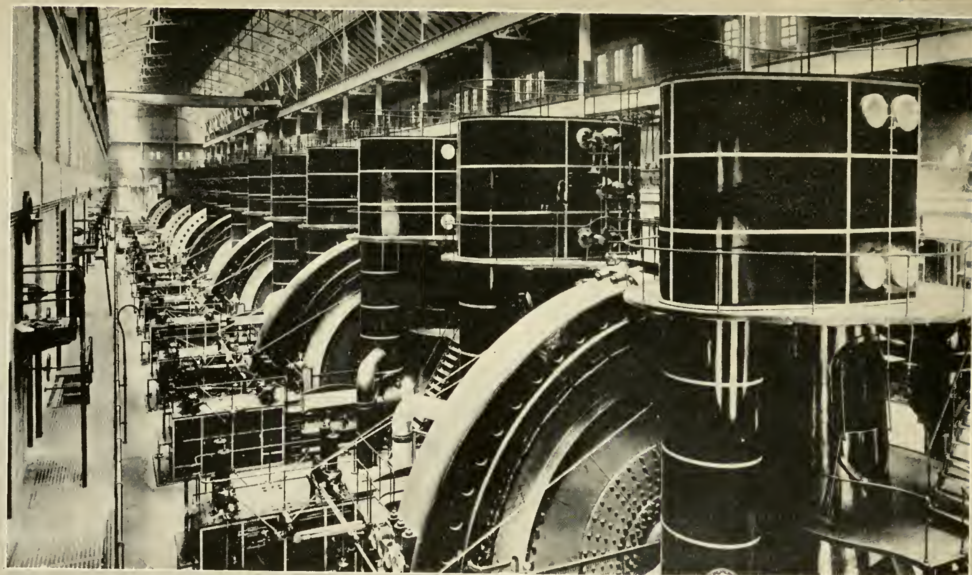
On the following Monday morning, Mr. Lange explained the situation to me and asked me what was the trouble. In this case the reasons were so plain that I immediately

told him that the inductive load represented by the exciting current of the shop motors had "killed" the field of the rotary, so that, naturally, it had run away. He said that he needed this machine in the power plant and asked whether I could suggest any way to prevent further trouble, since the condition of operation, as an inverted rotary, was one to be expected from time to time. I told him, off-hand, that what was needed was an excitation increasing rapidly with the speed, and I suggested that if we excited the field from a separate exciter, driven directly from the rotary itself, either by belt, or by an induction motor connected to the alternating current side of the rotary, an excitation would be obtained rising more rapidly than the speed; and that this would, automatically, tend to hold the rotary at approximately constant speed, regardless of the nature of the load. This was but an offhand suggestion, during a few minutes' conversation; but Mr. Lange immediately adopted it in the power house installation. It is of interest to know that this scheme is the only practicable one which has been developed for holding the speed constant in inverted rotary converters. This, like many other schemes described, was the result of a difficulty.

In the 1899 period, I find that several very interesting things were coming to the front. In this year, the Manhattan Elevated generator and sub-station plans, involving many new features, were worked out. The contract, placed in the latter part of the year, comprised eight engine-type alternating current generators, of 5000 Kw. capacity, at 75 revolutions, and, if I remember rightly, some thirty rotary converters of 1500 Kw. capacity, for the sub-stations. This practically sounded the death knell

of the direct current generator for railway work. The alternating current generators for this work were new, in the sense that they were not only larger in capacity than anything yet built, but very much larger in physical size, possibly larger than anything built since then. The generators were of the engine type, and stood about 40 feet high when assembled, and each one complete, weighed close to 1,000,000 pounds. Some of the large hydraulic plants of recent years have called for slow speed generators which have been very large and heavy; but these include such things as bearings, and bearing brackets, whereas the above given weight for the Manhattan generator is purely for the stator and rotor, without bearings, shaft, or other details. When this contract was being considered and the whole design was being worked out, I asked Mr. B. H. Warren, then General Manager of the Westinghouse Company, what limitation he thought would catch us the hardest in these machines. He said that it would be the limitations of shipping. I said that this was not so, as we had looked out for that part, but that we had no place in our shop where we could assemble these machines, as our highest crane would not clear the top of them when assembled. He laughed about it, but apparently took it seriously, for he had the matter immediately looked into with a view to building a new shop aisle with greater headroom.

These generators had the armature winding insulated with mica very much like modern machines; but the armature winding was of the built-up type, having partially closed slots, with the straight bars shoved through and with end connections then added. For 11,000 volt machines, this type of winding would now appear to be



The 5000 Kilowatt, Engine Type, Alternating Current Generators Installed in the 74th Street Power House of the Manhattan Railway.

Typical of installations before turbine driven units had been developed.

more or less prohibitive; but these windings stood the test of time with remarkable success.

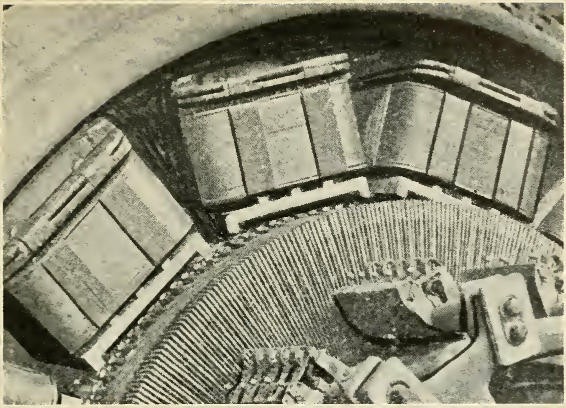
The field poles of these generators were equipped with copper dampers to assist parallel running, and to aid in smooth operation of the rotary converters. Mr. H. G. Stott, who was appointed Superintendent of Motive Power at this plant before the generators were installed, and was, therefore, not familiar with the effect of dampers on generators, anticipated considerable trouble in the parallel operation, until proper conditions could be determined by trial. He told me afterwards that he had expended about $4000 for a testing equipment in order to make rather complete tests before attempting parallel operation, but that while he was getting ready for this, his operators threw the machines together and went right on operating them as if there was no such problem as parallel operation. He always claimed that this was a good joke on him; but he also said that he believed the dampers on the fields had much to do with the matter.

The design of the generators began in the fall of 1899. About the same time, the design of the 1500 Kw. rotary converters was worked out. These were larger than any rotaries I had ever undertaken, and I carried the good features to what would now be considered an extreme. The designs were worked out for very low losses, and it is probable that very few machines at the present day are as efficient as these first big rotaries. Some of the precautions taken afterwards turned out to be quite absurd. For instance, some 2½ inches of wearing depth was specified in the commutators of these machines. This was at the customer's request. Some ten years afterwards, I asked Mr. Stott how much these commutators were

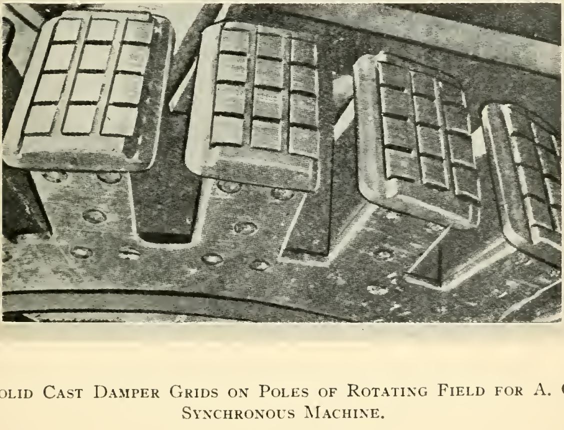
worn down. He said, that as nearly as he could determine, the total wear had averaged about 1/64 in. This meant about 1600 years' life for these commutators.

One interesting thing during the early operation of these rotaries, was that it was the intention to bring them up to speed by means of direct current from motor generator sets in each sub-station. Some of the rotaries were ready to operate before the motor generators were installed; and, in consequence, Mr. Stott brought them up to speed from the alternating current end, by starting up the whole plant together. Sometimes one of the rotaries would come up with reversed polarity. Mr. Stott attempted to correct this by reversing the field to "slip a pole." To his surprise, he found the machine would continue to hold in synchronism even with reversed field, and that the only way to reverse the polarity was to open the main alternating current switch momentarily. He said that this was the first time he had ever had experience with rotaries of such good synchronizing power.

In 1900 I apparently carried on only routine work. During this year the "cage type" damper was tried out on rotary converters, showing considerable improvement over the older form, and was soon adopted very generally. About the middle of this year, I was sent to Europe to visit the Paris Exposition, to note any new developments and to visit the various interesting power plants. This took up several months of my time so that my record shows less than usual of productive work during this period.



Solid Cast Damper Grids on Poles of Rotary Converter.



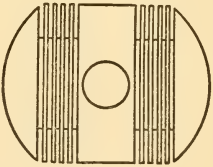
Solid Cast Damper Grids on Poles of Rotating Field for A. C Synchronous Machine.

CHAPTER VII

TURBO-GENERATOR DEVELOPMENT AND RAILWAY ELECTRIFICATION FROM 1901 TO 1919

IN 1901 we were rapidly approaching an important step in development, namely, the turbo-generator. It had been recognized, for several years, that the turbo-generator possessed great possibilities, but apparently no one appreciated how quickly it was going to take the field after it was once properly started.

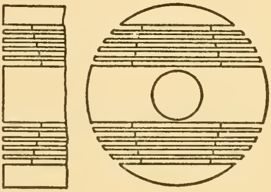
In 1898, as I have said, I worked out the design for a 1500 Kw. 6-pole, 1200 revolution machine. In the latter part of 1899, I undertook to design a 400 Kw. rotating field type, 2-pole, 3600 revolution machine. The rotor of this machine was of the parallel slot type, very much as those on later Westinghouse machines, except that the sides of the field were flattened off at the slotted part in order to allow easier machining and wedging. This rotor was the beginning of our slotted construction of fields, but was not in itself a commercial success, as the flattening of the sides resulted in an extremely noisy machine, by far the worst I have ever heard. We did all kinds of things to deaden this noise, without result. Also, the windage loss was enormously high, producing undue heating. Mr. Westinghouse was, personally, much interested in this rotor, for it ap-



Turbo-Generator Rotor with flat sides—this was noisy

pealed to him as possessing qualifications needed by high speed turbo rotating parts. After the tests, he asked me whether I could do anything further to help the machine, and I told him I would give it up *for the present.*

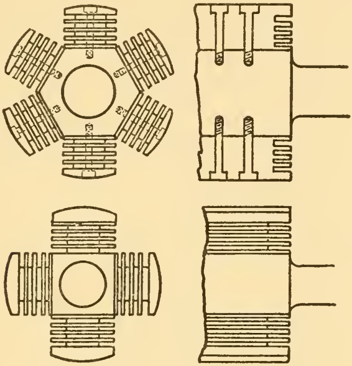
A few days afterward, it occurred to me that it was altogether unnecessary to flatten the sides of this rotor and that I could take a purely cylindrical core, slot it, and put in suitable supporting wedges in order to give a perfectly round, smooth surface. I then told Mr. Westinghouse that I was willing to start in anew on this construction and told him the idea I had in view. The solution was so simple that we both felt somewhat chagrined. He was so glad to see a possible solution that he told me to rush it through immediately. This rotor was successful, from the permissible noise and loss standpoints, and a large number of machines of this type were built later. This was the real beginning of the Westinghouse high speed turbo-generator work. This was in 1901.



Turbo-Generator Rotor with round sides—this was quiet

In 1902 and 1903, we were taking orders for 4-pole and 6-pole, 60-cycle machines and 2-pole, 25-cycle, some of very large capacity. For instance, in 1892 we began the construction of a number of 5000 Kw. 1000 revolution, 33-cycle turbo-generators for a foreign power plant. We ordered the cores for these machines from the Krupp Company, and a number of them were delivered to our works in East Pittsburgh. Upon test, however, the centers of these huge forgings were found to be glass-

hard and extremely brittle, contrary to specifications. Finally, we were obliged to build these machines of heavy



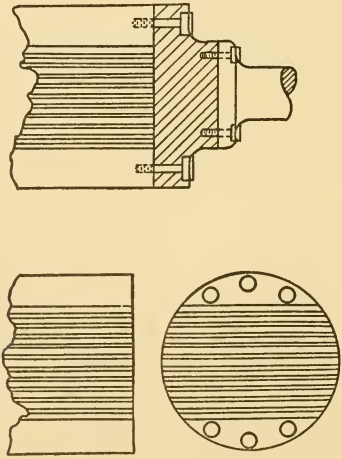
4-Pole and 6-Pole Turbo-Generator Rotor Construction with Parallel Slots

steel plates, from 4 inches to 6 inches thick, pressed on the shaft.

Finding that it was extremely difficult to obtain large forgings at that time, and as the plate construction did not work out as simply as we desired, we next went into the question of large steel castings for turbo rotors. There was a general condemnation of the use of this material, but after talking the matter over with me, Mr. Westinghouse, "backed me up" in my suggestion that we go ahead on such construction, especially as our method of slotting the cores for the windings permitted us to make a pretty good examination for defects, bad material, and other faults. All of these early rotors, both of cast and forged steel, were pressed on the rotor shaft. Quite a number of them loosened in time, so that sometime later

it was necessary to adopt the practice of heating and shrinking these cores on the shaft. This cured the trouble. Still later, the practice was adopted of casting the rotor cores and the shaft in one piece. This proved entirely satisfactory as long as the parallel slot construction of turbo rotor was used.

On the 2-pole turbo-generators, the first machines of the above construction were 400 Kw. On account of the presence of the shaft, it was not practicable to use nearly all of the available space on the periphery for slots; accordingly, the rotor was only a little over half as effective as it might have been had the shaft not inter-



Turbo-Generator Rotor with bronze end caps, also shows bolted on stub shaft

fered. This led to a radical step in the construction; namely, the building of a 2-pole rotor without any shaft,

and with the slots and windings covering a much larger part of the surface. We then added bronze "end caps" which carried the shaft stubs. It was felt that it was necessary to go to some construction of this sort in order to increase the capacity of our 2-pole machines, both in 60 and 25 cycles. A 3600 revolution machine of this type was constructed for 1000 Kw. capacity. On test, this machine did so well that we found we could rate it at 1500 Kw. with the addition of more copper in the armature. This was a long jump in capacity, owing to the change in construction; and from this on, the ratings grew rapidly until we were building approximately 4000 Kw. at 3600 revolutions.

In the 25-cycle, 2-pole machines, similar results were obtained by bolting on bronze ends. Capacities rose quickly from 2000 Kw. to 5000 Kw. and then 10,000 Kw. which at the time, was supposed to be as big as would ever be needed. Thus, about seven years after we really had begun building the rotating field turbo-generator, the capacity had risen to 10,000 Kw. which was a pretty rapid growth.

The engine type alternator had reached its zenith about 1901 and 1902. By 1903 and 1904, the power station people had reached a point where they believed that the turbo-generator was coming to the front so rapidly that they hesitated to install engine type machines.

In consequence, this type of machine did not persist where units of large capacity were required. As the turbo-generator design and construction was so radically different from the engine type generator, very little of the tremendous amount of accumulated experience in connection with the latter type of machine was available

for the former, so that we were very much in the same position as if starting in anew, from the ground up.

It seemed a pity, after I had spent so many years in the development of engine type generators, that I should have to turn in and help undo this work. However, it has always been my practice to follow up what I considered the most promising line of endeavor; and as the turbo-alternator held out greater promise than the engine type, I naturally felt it was my duty to push it to the limit. In one way, I believe I can properly claim considerable credit in connection with this turbo-generator development; i.e., in the race for higher speeds, I have been in the forefront, as I have always advocated that, sooner or later, we would go to the ultimate limit, and that, therefore, we might as well do so as quickly as possible. I think that most of the criticism which I received in such development was on account of my tendency toward higher speeds. But then, as I said, away back in the beginning of this story, I always liked high speed.

In 1901 and 1902, much discussion developed regarding the feasibility of extending the use of the ordinary 600-volt railway systems to heavy traction service. It was becoming well recognized, at that time, that, even with the assistance of the polyphase transmission and rotary converter, the existing electric railway systems had definite limitations. The collection of current by overhead trolley on heavy equipments and for high speed cars presented serious difficulties. The third-rail system was being tried with a view to overcoming the current collection difficulty, as it was well recognized that this was a serious problem.

In 1901, I took up actively the question of building

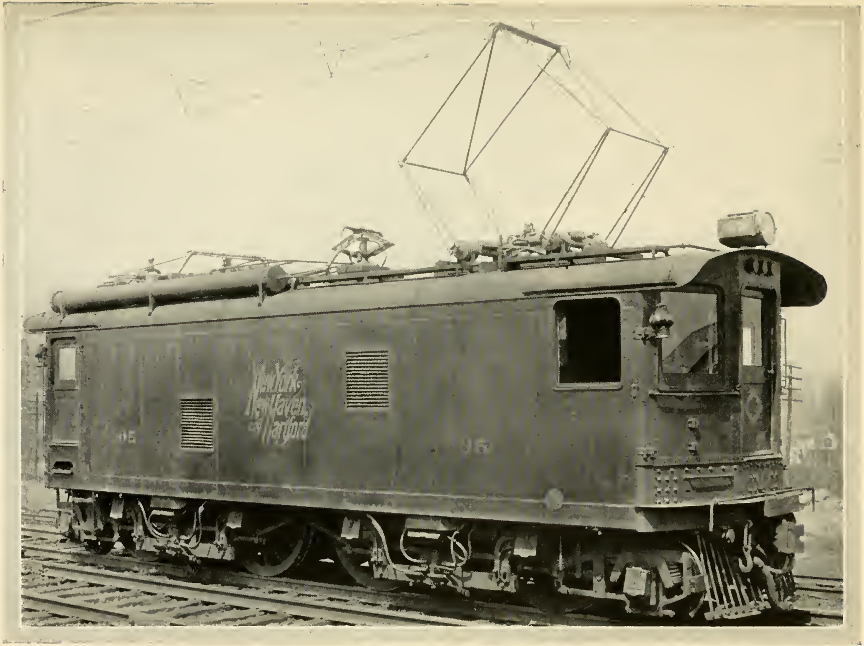
single phase, commutator type motors. It was not the discovery of an alternating current commutator motor with series characteristics which led up to this work, for such motors were known long before, and I had already —some four years earlier—built 60-cycle series motors up to 40 horsepower for the Swift Company's cranes which operated pretty well. It was the *need for a higher voltage, smaller current* system that led to this development. The commutator type alternating current motor had long been considered by me as merely a possible means for accomplishing the desired result. In other words, I had simply considered it one element of a possible system. In fact, I never looked upon the single phase, commutating motor as possessing inherently any particular advantages over other types, fully recognizing that, in some ways, it would always have certain disadvantages; but I have considered that the general advantages of the single-phase system, as a whole, might well outweigh the disadvantages of one or more of its elements.

My experience and data on 60-cycle commutator motors, just referred to, indicated very clearly that, with the limitations of commutation which I had encountered, I could make very respectable-sized motors at much lower frequency, these even coming within the range of railway practice. Therefore, a motor for $16^2/_3$ cycles was designed, in 1901, for railway service; and, after tests were made, a contract was taken for a road between Washington, Baltimore and Annapolis. This was described in an A.I.E.E. paper in 1902 and was the first authoritative description of the single phase system as later developed. Following this publication, the devel-

opment of this system was taken up in both America and Europe, but the motors were usually along slightly different design from the one which I had followed.

In 1903, we began to take orders for 25-cycle equipments, mostly for interurban roads with infrequent service. In fact, this system apparently met a need for unusual "promotions," such as new roads through more or less undeveloped territory with infrequent service. Consequently, a number of roads purchased this type of equipment for relatively short stretches (ten to twenty miles long) in which future extensions of fifty to one hundred miles were contemplated. In many cases, these future extensions never developed, not primarily on account of limitations of the single phase system, but because, in the first place, they were impossible enterprises. In other words, the promoters endeavored to turn unpromising developments into promising ones by the installation of a new system of electric transportation.

The first very big enterprise undertaken with this system was in connection with the electrification of the New Haven Railroad. This project was taken up in the latter part of 1905, becoming quite active early in 1906; and contracts were taken for a large number of locomotives to operate by single phase. These first locomotives were designed to use gearless motors. This was an exceedingly big undertaking, as everything was absolutely new from the generating plant to the transmission system, even to the minor details of the locomotives themselves. We had built single phase generators before, but they were of much smaller capacity. In the generators themselves, which were wound directly for 11,000 volts, with one terminal grounded, we met



First Type of New Haven Locomotive.

trouble after trouble, owing to such things as abnormal voltage stresses, effects of corona on windings, and effects of the pulsating armature reaction of the field construction. Massive cage dampers were then installed on the rotors to suppress pulsations; the insulations on the winding were replaced by mica to overcome corona effects; and various other improvements were developed from time to time, till we thus got "out of the woods" as far as the generators were concerned.

In the transmission system, equally serious troubles developed, due largely to the excessive number of short circuits on the 11,000 volt overhead system, resulting in tremendous voltage disturbances when the breakers were opened. Furthermore, the addition of the massive dampers on the generators made them quite "vicious" as regards short circuits. When the first generator was installed and was in successful operation, line troubles did not appear to any extent, as apparently a small capacity in generating plant did not give harmful short circuit conditions. However, we did not fully appreciate this; and when the second, and afterward the third, generator was thrown in on the system, real trouble began. Breakers "blew up" right and left; the numerous shorts had fearful effects on the generating plant in the way of distorting the windings, pulverizing the insulation and all kinds of unexpected troubles. Tremendous "fireworks" would come from the machines themselves at such times, but afterwards, upon examination, no visible signs of burning would be discovered. This condition greatly puzzled me.

Finally, one day, while examining dismantled end housings, I discovered that the joints between the hous-

ings showed evidence of fusion. This then led to the discovery of the real cause of the fireworks in the machines. In the case of violent short circuits on the line, heavy secondary currents were set up in the iron end housings by the armature end windings, while the tremendous shocks on the machines were such as to break contacts in the joints between the housings. Thus, there would be fusion of the metal edges of the housings, and with high air pressure inside, this would show as fireworks on the outside. In other words, the display of fireworks was not from the inside of the machine, but from the joints in the housing. It was quite a relief to me to discover this.

With two or three generators on the system, the New Haven transmission system troubles had become so bad that a special "selective" system of trolley operation, protected by resistance breakers, was developed by Mr. H. P. Davis and Mr. Frank Conrad, who were handling this work. This method was first tried out in a very crude way; but showed evidence of such merit that it was installed in a permanent form as quickly as possible, and proved to be of tremendous advantage in the operation of the New Haven system.

In the locomotives themselves, the most trouble was feared from the motor operation. The motors were designed under my own personal supervision, and every bit of available data was analyzed most carefully in order to take the utmost advantage of all the experience that was then available. An interesting statement which may be made in regard to these New Haven locomotives is that practically all the troubles that developed were at unexpected points. Wherever we really expected trouble, it occurred only to a slight extent, or possibly not at all.

As the locomotive motors were of 300 hp. and of very low speed, and mechanically several times larger than any single phase motors previously built, naturally we had feared trouble in these more than elsewhere; but they gave a most excellent record, many of them running up into hundreds of thousands of miles before the commutators required turning. One serious difficulty, at first, occurred at a point where no one expected it; namely, in the thermal capacity of the control system. As the motors were of the gearless type and of very high thermal capacity, owing to their low speed and consequent size, they could carry very heavy currents for a considerable time without undue heating. Consequently, in many cases, the motors were loaded to a higher limit than intended. The control system could handle these heavier currents without any such trouble as undue burning at contacts; but the parts were unduly overheated in the long heavy current pulls, owing to a relative lack of thermal capacity compared with the motors themselves.

There were a number of other difficulties in the locomotives, and at first, a certain amount of trouble from the 11,000 volt locomotive transformers was encountered; also, the trolleys had to be remodeled, to a certain extent; but, taking it all in all, these locomotives made a very creditable record, and are still counted as "good old wheel-horses" by the New Haven Railroad Company.

This New Haven system was very much criticised, and in most cases quite unjustly. The apparatus has worked, and worked well. Looking back on the development, it is now obvious, from our present knowledge of the problem, that this installation represented much excellent engineering work. Mr. McHenry, Chief Engineer of

the New Haven Railroad, and Mr. Murray, Electrical Engineer, went into the matter in a whole-souled, broad-gauge way, which was of primary assistance in getting the system into good operating shape. If they had been disposed to be "nasty" in the early stages of the work, they probably could have put the whole matter "to the bad"; however, with their full cooperation, it was turned into a success. Later, the Harlem Yards of the New Haven system were electrified, with great success, as there developed practically no trouble of any sort. Still later, the road was extended from Stamford to New Haven, and the capacity of the system, in the way of additional power stations and car equipments, has been increased enormously.

In all history of electrical engineering, there has been possibly no undertaking that has exceeded the New Haven; and yet very many engineers of good standing have gone out of their way to try to bring it into disrepute. This has hurt my feelings almost more than anything else; for it has always seemed to me that anything in the way of successful accomplishment in the engineering field tends to advance the art; and that any true engineer should speak well of any great success. A certain noted foreign engineer visited this installation a very few years ago and he afterward said, in effect, "Apparently, you people have but little conception of what you have accomplished, judging from the talk which I have heard and the fact that so little data has been published."

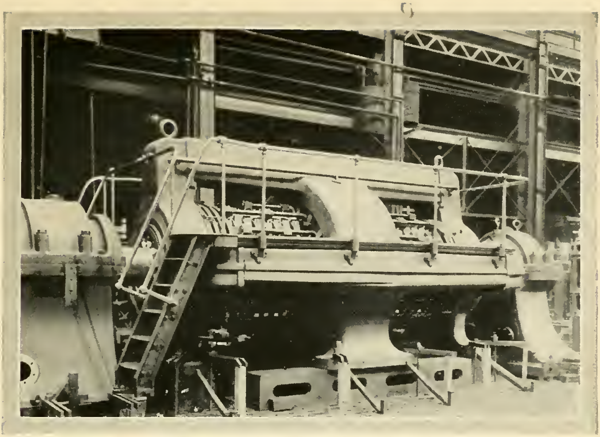
Beginning about the same time as the New Haven work, we had undertaken another railway electrification, namely, the Sarnia tunnel under the Detroit river.

This was not only a tunnel installation, but it was "the neck of the bottle"; moreover, on account of danger from smoke fumes, it was desirable to electrify it. In 1906, this was undertaken and five freight engines were constructed with the geared type, single-phase motors. This involved, relatively, few new and radical problems, compared with those involved by the New Haven; and, therefore, did not attract anything like as much attention as the latter. However, it has been extremely successful since the first.

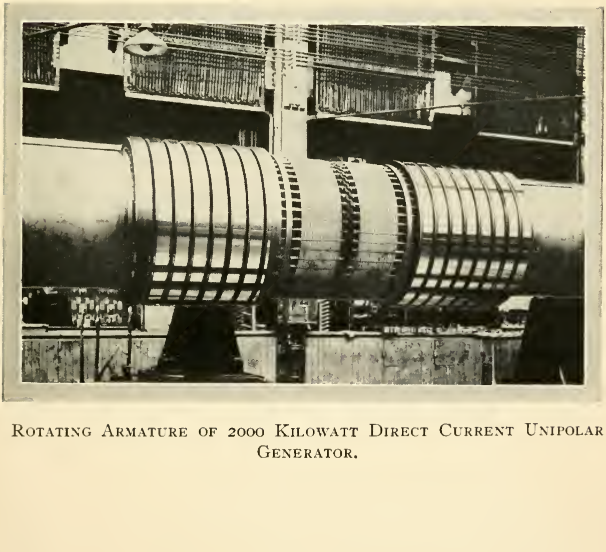
In the period during 1904, 1905 and 1906, I did not get a chance to do as much engineering as I desired. In the latter part of 1900, I had been appointed Assistant Chief Engineer of the Company. This did not change my duties at all, as I still spent practically all of my time on engineering work, principally design. In 1903, I was made Chief Engineer; and, in consequence of this, for the next two or three years, a considerable portion of my time was taken up with executive work, which interfered, to a certain extent, with my engineering. I still managed, however, to keep in close touch with the New Haven work and with the turbo-generator development. After two or three years, as it had become obvious that I was losing ground in the engineering work, I was relieved of the executive end by the appointment of Mr. Davis as Manager of Engineering, so that thereafter, practically all of my time has been taken up on engineering matters. During this period, commutating poles on direct current apparatus came to the front and I did much in the analysis of commutating conditions, which culminated a few years later in a paper on "A Theory of Commutation" presented before the A.I.E.E.

Also, in 1906, the uni-polar generator was attracting much attention as a possible solution of the d.c. turbo generator requirements. The General Electric Company had built a number of moderate-sized uni-polar machines and had contracted for one of 2000 Kw. capacity. About 1897, I built a small uni-polar machine for meter testing. Quite a number of these machines were built both for the American and the foreign Westinghouse companies, and even for some outside instrument companies. About 1904, I made an extensive series of tests on the collection of heavy current on high speed rings, for the purpose of determining the possibilities of current collection for uni-polar generators. These tests indicated quite promising possibilities. However, as we learned afterwards, our tests were stopped just a little bit too soon; for, had we continued our runs for two or three days longer, we should have discovered certain serious difficulties in current collection, which were a function of time. These we really did not encounter until we actually built a machine.

In 1906, we took a contract for a 2000 Kw. 1200 r.p.m. 260-volt, uni-polar generator, which was the first and the last large machine of this type that the company ever undertook. A history of this development has been given in an A.I.E.E. paper, entitled—"The Development of a Successful Direct Current 2000 Kw. uni-polar Generator." This description was so complete, in that it went into the difficulties encountered and the remedies found, that nothing further need be said on this work. The only thing that might be worth giving is that part of the story of a period later than that covered by the Institute paper. This machine continued in successful operation for several years and was finally closed down,



2000 Kilowatt Direct Current Unipolar Generator.



Rotating Armature of 2000 Kilowatt Direct Current Unipolar Generator.

because of the fact that the customer was able to purchase polyphase power at such good rates that it apparently was cheaper to shut down his own plant and operate on purchased power with induction motors. The original direct current plant was held in reserve for a considerable period, but I have not heard in recent years what became of it.

The principal reason for dropping all further work on the uni-polar type of machine was not the operating difficulties; for, as shown in the Institute paper, these were overcome. However, the developments in turbo-alternator construction and in rotary converters, principally in the direction of high speeds, were such that better economies could be obtained at less total cost of equipment than with the uni-polar type. Furthermore, the combination of the high speed alternator and the rotary converter gave some very considerable advantages in flexibility, as direct current could thus be produced by a machine located very close to the load, while the turbo alternator could be located at any other most convenient point. These reasons, of course, were sufficient to throw the uni-polar development into the discard. However, while it lasted it was an interesting and *exciting* experience; and a very considerable amount of valuable data was obtained, which has since been used in other classes of apparatus.

Also, in 1906, we undertook our first big reversing steel mill project. This first outfit was of very much the same kind as the present ones, except that we tied up all our d.c. generator equipment in one big double commutator machine. This generator was of about 3000 kw. capacity and operated at a normal speed of 375 revolu-

tions. It had both commutating poles and compensating windings, and the armature winding was of a special type, giving the equivalent of a half turn per coil. Taking everything into account, it was a very unusual machine. The only serious difficulty in this machine, at first, was that it was entirely too much over-compensated. However, as it was installed and put into operation just before the panic of 1907 culminated, when all business was very hard hit, it did not get enough real service during the following year or two for us to find out definitely what to do with it. Finally, when the steel business recovered, an engineer was put in charge to determine the proper adjustments, and he soon corrected the over-compensation difficulties. The customer's engineers considered the results of the operation of this generator so important that they went to the expense of purchasing a duplicate armature in order to have it always available, in case of an accident, to replace very quickly the damaged armature. They estimated that the value of the output of this reversing set per day was so great that the prevention of a comparatively short delay in the service would result in benefits that would more than pay for the cost of a new armature.

This big reversing mill set was the predecessor, in this country, of all the later sets of this sort; and it still holds its own with later designs. This set antedated other sets by a period of several years. This was principally due to the fact that the financial conditions of 1907, and the following two or three years, delayed any large developments. I put in much hard work on this set, along with the other engineers who were working on it, and I was quite proud of the results obtained.

The next few years, commutating poles came into very general use on both d.c. generators and rotary converters. In 1910 I prepared a paper for the A.I.E.E. on the advantages and disadvantages of commutating poles in rotary converters. This paper not having really been prepared for the Institute, but for the use of some of the younger engineers within our organization, was completed for the Institute only under an emergency call from its Meetings and Papers Committee. It showed that, with rotary converters of the slow-speed type then existing, there was but little advantage in the use of commutating poles. However, it was brought out in the discussion, that the real field for commutating poles in rotaries would be in the increasing of the speeds so greatly that the usual commutating limits would be greatly exceeded, in which case, commutating poles would be of direct advantage. It is interesting to note that within a year or so after this, rotaries of practically double the usual speeds were coming to the front; and in these, of course, the commututing poles were used.

Along about in 1907 and 1908, the 60-cycle rotary again began to come to more general attention. Turbo-generators were being used extensively, and this made the stability problem much easier. Improvements in dampers on the rotaries helped still further; but the greatest improvement was made by increasing the peripheral speeds of the armature core and commutator about 20%, thus allowing a very great reduction in the flashing characteristics of such machines. These various changes so greatly improved the performance and efficiency of the 60-cycle machine that it became very closely comparable with the 25-cycle design. Further progress of

the 60-cycle rotary has been steady since 1910 and 1911.

In 1913, a new development came along in the Norfolk and Western Electrification. This matter had been under consideration for several years. In the latter part of 1913, negotiations became quite active and a contract was closed for what constituted an almost new system. As this electrification was a mountain grade problem and was "the neck of the bottle" for the Norfolk and Western freight business, the electrification was undertaken for the purpose of greatly increasing the capacity at this "neck." Single phase was adopted for the system, and the locomotives were equipped with 3-phase induction motors operated by the equivalent of a 2-phase machine acting as a phase converter, transforming from the single phase supply system to 3-phase at the motor. The motors were originally designed for three speeds to give 28, 14 and 7 m.p.h.; but somewhat later the 7 mile speed was eliminated, in order to simplify the locomotive. The 14 and 28 mile speeds were obtained by single windings on the motors adapted to give a 2 to 1 combination of poles.

This fundamental idea of using 3-phase motors on single phase circuits was not new. As far back as 1897, I had made tests on two 100 hp. experimental traction motors, using one as a phase converter and the other as a motor to transform from single phase to polyphase; this was simply to determine the possibilities. Consequently, the Norfolk and Western was really an extension of an old experimental test, as far as our data was concerned.

This Norfolk and Western electrification involved many radically new problems. The scheme, as a whole,

Norfolk & Western Locomotive.

was well known and was well understood, but the construction was practically new. Incidentally, one of the neatest elements in this scheme occurred to me while I was waiting for a train in the Pennsylvania Station in Pittsburgh on my way to take a vacation for a couple of weeks. The improvement appeared to be so important that I called Mr. Feicht by telephone and explained the whole scheme so that work could progress on it while I was away.

The generator scheme presented many new problems from the fact that 2-pole, 1500 r.p.m. single-phase turbo-generators of about 11,000 to 12,000 kv-a. capacity were required, which were much larger than any single-phase machine heretofore built. This involved new problems in damper construction and other details which required our utmost ability to solve. Also, the phase converter itself involved many new elements. Great precautions were at first taken to prevent undue unbalancing of the 3-phase circuit relations; but we had not progressed far when we found that only two steps in the balancing were of any practical use. As the motors were controlled by resistance, liquid rheostats were used for this purpose, the liquid being operated by pumps. This was, in general, not a new scheme, as the Giovi 3-phase locomotives, built by the Italian Westinghouse Company, already had used such liquid rheostats. Consequently, we had a certain amount of previous experience to fall back upon.

Considering the tremendous service that this road had to handle, most of the electric traction work, which had been done previously to this, could be placed almost in the "toy" class, as far as heavy loads were concerned. Throughout, new conditions were met, everywhere, even

in the mechanical parts of the locomotives themselves, owing to new and hitherto unknown and unsuspected conditions brought on by the excessively heavy service. From the locomotive and the power house considerations, it is probable that this Norfolk and Western Electrification presented fully as serious problems as the original New Haven, which I have described. Although I was only one of many working on this proposition, yet I had plenty to do for several years.

In recent years, there has been tremendous development in large turbo-generators, in which the Westinghouse Company has had its share. I have had considerable to do with this work throughout, especially whenever we exceeded the limitations of preceding designs. About 1910, the company began to change over from the parallel slot type of rotor to the radial slot, as certain limitations of the parallel slot type were being reached. Mr. Behrend was very ably following up this construction for us, and all of us who knew anything about such machines were kept more or less busy in tackling new problems which came up.

For several years, I was, also, in close touch with the experimental work on the metal mercury rectifier, including the trial of a rectifier car equipment on the New Haven system, to determine its possibilities. Some of the later work with which I have been in close touch has been the application of reactances for protective purposes, speed control of induction motors, and electric ship propulsion, which is just now coming into great prominence.

CHAPTER VIII

OTHER LINES OF WORK

IT is apparent, throughout this story, that I have had less to do with transformers than with many of the other kinds of apparatus. The transformer work was very early in the charge of C. F. Scott, and, later, J. S. Peck. I had practically nothing to do with the design of such apparatus, although I was continuously in touch with the engineers in a more or less consulting capacity, especially in the case of application of transformers to other apparatus, such as rotary converters, induction motors, single-phase equipment and the like.

Also, in what we usually call "detail work," covering measuring instruments of all sorts, relays and protective devices, such as lightning arresters, I have only been in touch in a consulting capacity from time to time, for such work, for many years, was conducted by Mr. H. P. Davis, and there was no occasion, whatever, for more than a working connection with it.

In regard to research work, I may say that I have been connected with it, in one form or another, ever since employment in the company. In the very early days we did a relatively large amount of research work in the form of experimental shop tests. Our early insulation work, particularly, was very largely a research problem. Mr. C. E. Skinner, formerly head of our Research Department, began, back in the early '90's, on insulation work to

help out the other departments, and, as a result, he has been identified with this work most intimately. It finally became his special department and has grown to be a very large and important part of our work. I have been closely associated with Mr. Skinner in his work, especially in the insulation part, almost from the time he came into the company. In fact, Mr. Skinner came into the Westinghouse Company in the summer of 1890 upon my recommendation to Mr. Lange, who wanted a man of his general characteristics.

This ends the purely engineering side of the story; but there are some other lines which will be discussed, to a certain extent, such as educational work, preparation of technical papers and patent matters, also, possibly, some of my own purely personal lines of endeavor, which might be interesting as "sidelights."

INVENTIONS, PATENTS, ETC.[1]

As will be noted throughout this story, I have made a number of inventions and have taken out many patents. It may also be noted, from the general trend of the work, that many of these inventions were made in attempts to overcome some serious situation. In some cases, the patentability of the ideas did not occur to me until the results obtained by actual trial had proved the merit of the idea or scheme. In other words, as has been said by someone, the patents could be considered largely as "by-products" and, therefore, more or less incidental to the other work.

I have had some interesting experience in attempting to

[1] Mr. Lamme took out 162 patents in all. See review in Appendix.

obtain patents. For instance, one of my earliest suggestions, put in the form of a patent application, involved the use of a waterwheel, or hydraulic prime mover, without governor, directly coupled to a series generator also without any regulating devices, for the purpose of obtaining constant current for a transmission system, such as the Thury. The Thury installation, then existing, had governors on the waterwheels to hold certain speed conditions, and governing apparatus on the generators to hold constant current conditions; whereas, I claimed that both sets of governing mechanisms could be omitted. This particular application "hung fire" in the Patent Office for several years, being finally rejected as embodying no invention.

An interesting commentary on this decision is that some ten or fifteen years later, in connection with a description of one of the newer Thury generating plants, particular attention was called to the fact that the whole generating outfit had been very greatly simplified by omitting the regulating appliances, to a great extent, from both the hydraulic and electrical machinery, the statement being made that it had been found that, without such regulating appliances, the combined unit tended automatically to give the required constant current condition. In other words, the principal makers of such systems considered the idea more or less novel, some ten years after my application had been rejected.

Again some years later, my application covering the use of leading currents from a synchronous motor, to correct the voltage relations in a transmission system, was rejected because it was *impossible* for the current to lead the e.m.f. in an alternating current system. The

error of this decision was later perceived, and a patent was obtained. A rather common experience which I, together doubtless, with many others who have been involved to any extent in patent applications, have had, was finding that what appeared to be more or less fundamental schemes, had been patented years before, and yet had never been used. In one case, in one of my applications for a certain quite valuable improvement in the Westinghouse large d.c. generators, it was found that the idea had been patented about forty years before; that is, long before large d.c. generators were even contemplated; and moreover, that this early patent showed pretty clearly the particular features that I claimed.

Again it has happened, not infrequently, that apparently good patents could have been obtained, if they had been applied for, on features which I supposed were old and well known. In other words, I assumed them to be almost self-evident.

It is surprising too, at times, to note how many people think of almost the same thing at the same time, and how their ideas are yet, unquestionably, independently conceived. One instance occurred in my experience of four different people coming to me, within about one month, with the same scheme, to ask my opinion. Upon investigation, I found that they all had devised the scheme independently; and what is more interesting, it was not a scheme for which there was any particular call at that time. In my own experience, I have many times run counter to some of my associates or friends on what proved to be almost an identical idea. Usually with those working in the same organization, there was not much trouble in straightening out such matters, without

going to the trouble of declaring patent interferences.

During my whole experience in the electrical engineering business, I have found that a more or less general knowledge of the patent situation is of great value, not only for the sake of knowing what may be patentable, but also in order to avoid any infringements of patents of others. I have found however, in many cases that if I undertook a problem with a view to carrying it through to complete success, I have automatically avoided existing patents. In other words, if my solution were more successful than the existing methods, the chances were that it was novel in a number of its fundamental features.

The question of "dodging" patents is often a very serious one. At times the doors seem closed in whatever direction we turn; and this sometimes means that we have to start out in an entirely new direction in order to reach the desired result.

From its organization, some years ago, until quite recently, I have been on the Patent Committee of the Westinghouse Company, and have been unfortunate enough to be selected as its Chairman, involving work which I never have liked. The function of this Committee is to pass upon patents and inventions submitted to the Company, either from within the Company or from without, with a view to determining their possible value from the points of view of sales, manufacture, engineering and policy.

TECHNICAL PAPERS

As far as my technical writings are concerned, my personal opinion is that these might almost be classed under

educational work; for nearly all of the papers which I have prepared for publication have been written, very largely, from the educational point of view; in fact, from time to time during the past thirty years, I have prepared a number of technical papers and statements purely for use among the younger engineers associated with the company.

As explained in the first part of this story, some of my early work was to prepare technical explanations of our electrical apparatus for Mr. Schmid. These early papers, while very crude in themselves, were largely of an educational nature. By considerable effort they were put into such form that they could be quickly and easily read by an overworked man, like Mr. Schmid. Undoubtedly, this early experience has been of appreciable help in later work. I found long ago that most readers of technical papers, except a few specialists, college professors and others of similar type, would not read anything of a mathematical nature. If they saw a page with a mathematical formula or two on it, they would turn to the next page, thus losing the gist of the argument and possibly not absorbing what they found there. In consequence, they would thus pass the article by with but little attention.

I had a good illustration of this distaste for mathematical formulæ in the preparation of my paper on the polyphase motor, written in 1897. In this I undertook to explain in a general way the characteristics and operating features of the Westinghouse type "C" motor, then a new and much maligned piece of apparatus, as it was a cage wound motor with a large starting torque which was contrary to general opinion at the time. I worked, off and on, for some months, on this paper getting it down

into reasonably simple form, except that here and there was a formula or equation which I saw no way to omit. Upon my submitting the first copy of this paper to Mr. Skinner for review, he told me that it was all right except that he had not read the part that had mathematics in it; and he asked me if I could not leave out all of the mathematics. I did not believe that this was possible, but I thought that if Mr. Skinner would not follow the mathematical part, it was hopeless to think that the majority of others would do so. I then went at it again to see whether I could lessen the *visible* mathematics; and I considered it quite a triumph when I was able to eliminate all of the equations and yet tell practically what I meant to tell, in ordinary English. The correctness of this policy was proved later by the number of favorable comments I received on the paper, most of which were to the effect that the paper was good because it had no mathematics.

I have often used this illustration in insisting that mathematics be eliminated from the Company's papers except in a subject which was essentially mathematical. In one case, which I recall, Mr. P. M. Lincoln was writing an article explaining the compounding of rotaries by means of reactance in the a.c. leads. He wrote a very good explanation except that he included vector diagrams. He then asked my opinion of it and I told him it was all right, except that an ordinary reader would not understand the diagrams; and I suggested that he explain the matter without them. He said it could not be done. I told him to go ahead and make the attempt, adding that, if he could not succeed in doing it, I would then do it for him. He said he would not stand for that, so he re-wrote that part of his paper without vector diagrams and showed

it to me again. I asked him what he, himself, thought of it. He said it was his own private opinion that it was very greatly improved. I have cited this case a number of times to our younger engineers as a good example to follow.

In the last fifteen or twenty years, I have written possibly thirty or more technical articles, mostly on subjects involving electrical principles and design. About half of these, prepared for the A.I.E.E., have appeared in the Proceedings from time to time. None of these contain any great amount of mathematics, although, in two or three instances, the subject did not admit of purely non-mathematical explanations. Some of these papers have been reprinted in the *Electric Journal* and others have been re-issued by the Westinghouse Company as technical literature for its customers. Also, I have written for the *Electric Journal* a series of histories of the development of certain lines of apparatus, such as the alternating current generator, the direct current generator and the electric railway motor. My principal idea in writing these was to furnish a brief record of some of the company's earlier work, for the information and use of the younger men in the organization.

I have always found it more or less difficult to prepare technical articles, or any other articles, for that matter. The routine of revising, perhaps time after time, the wording of the paper in order to bring out more clearly the ideas I had in mind, has always been very irksome. Moreover, in many cases, when I have tried to explain a thing clearly, I have found that I could not do so because I, myself, did not have a properly clear conception of it. Naturally, under such conditions, I could not expect to

make anyone else see the point. This is one thing that makes technical writing so difficult for me.

One of the duties which has gradually been "loaded" upon me has been to approve all of the technical literature (other than mere advertising and company publications), which is prepared by people connected with the Westinghouse Company. In some cases, such approval is mere formality, as I know what is being covered by the paper during its preparation. In other cases, it is necessary to go into the article in considerable detail, especially to see if it is well written and if the ideas are clearly expressed. Also, I had to pass upon some of the technical points. In case of the younger writers, it is necessary to study the writing from the point of literary style, and to call the writer's attention to desirable improvements. This work is sometimes quite interesting, but it does certainly take time.

As a member of the Publication Committee of the *Electrical Journal*, practically all articles prepared for this paper are referred to me for approval, before publication, particularly to have the technical points passed upon. The hardest part of this work, is in connection with the answers to the questions in the Question Box which the *Journal* has been carrying on for many years. Most of these answers are written by engineers who are more or less specialists on the subjects which they attempt to cover in their replies. Sometimes the questions are most difficult. It requires careful watching to see that the reply covers properly and simply the answer to the question. It is surprising, at times, how many different meanings our engineers can obtain from a given question. This technical work has proved to be a most excellent

training for our younger men, especially in the way of making clear and easily understood explanations. In these replies, I encourage the younger men to couch their ideas in language to correspond, as far as they can determine, to the technical capabilities of the writers of the questions. For instance, if the question indicates that its writer is not experienced in mathematics in any form, it is obviously absurd to include vector diagrams or mathematical equations in the reply. I have endeavored also, as far as I could, to hold the *Electric Journal* up to the highest standard. I have considered, and my associates of the Publication Committee have agreed with me, that the function of the *Journal* is *educational* primarily, and not *advertising;* and that its articles should be dignified in tone and should represent as good English as its many contributors can write. At times, it has proved quite difficult to maintain a sufficiently high level.

A.I.E.E. WORK

As I have said, under the subject of "Technical Papers," a good percentage of my technical writings has been presented before the A.I.E.E. principally during the last ten years. As far as Institute work proper is concerned, I have never been closely associated with it, except during a three-year period as a member of the Standards Committee. Some seven or eight years ago, arrangements were made to begin a set of new standardization rules, to be issued by the Institute. Dr. Steinmetz, having been made a member of a special revision committee, asked me as a favor if I would not join him in this work; or to put it another way, he told the Standards Com-

mittee he would take up this revision work, if I would join him in it. In consequence he asked me to take it up. Up to this time I had not been closely in touch with this standardization work but I agreed to do what I could.

At the first meeting of the Standards Committee, in which the proposed revision was discussed, my opinion being asked, I said that in all former revisions of the rules, the committee had merely accepted and approved existing practice, but had not in any case, as far as I knew, attempted direct revision along the lines of future practice based upon fundamentally correct principles. In other words, they had not taken into consideration well indicated tendencies. I claimed that in consequence, the rules were always behind the times and would never be up-to-date unless they did take into account well pronounced tendencies.

Two Sub-Committees were named, consisting of Dr. Steinmetz and me on Temperatures, and Messrs. Merrill, Robbins and Powell on ratings. These took up the problems of revising the general subject of capacities and ratings as fixed by temperatures and other limitations. In this work, a number of meetings of these Sub-Committees were held, at Schenectady and East Pittsburgh, as well as at New York, in order to delve more deeply into the available data so that we could decide on some fundamental basis for fixing the capacities of apparatus. In this work I believe I was the first to bring out the idea that we must use, as a starting point the *ultimate temperature* which any insulation will stand, and from this work back through the temperature drops in the insulations to the *observable temperature*, upon which the capacity

should be determined. This at first was considered quite radical and most questionable, as it was not based primarily on *temperature rise*, which was then well fixed in the minds of the public. However, this idea was further developed and expanded and has now finally been pretty thoroughly accepted throughout the world.

As a result of the above, it naturally followed that different ultimate temperature limits should be taken for different types of insulations; and I believe I was the first to bring out the fact that with mica insulations very much higher temperatures should be chosen than with fabrics.

I was on this standardization work for three years; and in the third year at the first Midwinter convention ever held by the Institute, lasting three days, the entire series of meetings and papers covered the varied problems of proposed standardization, which it was desired to put before the electrical public before including them in the new rules. Many of the suggestions put forward were fought bitterly but much valuable information and criticism were obtained; and it was only a relatively short time until the new standardization rules were brought out and accepted quite generally. This ended my connection with the work as I asked not to be considered in connection with the Standards Committee work during the following year. Dr. Steinmetz also ended his connection with it, at the same time.

NAVAL CONSULTING BOARD WORK [1]

In 1915, following the request of Secretary of Navy Daniels, that certain designated technical societies each

[1] See Appendix, same subject.

select two members for a proposed "Naval Advisory Board," I was one of the fortunate ones to be nominated from the American Institute of Electrical Engineers, along with Mr. Frank Sprague. I have always looked upon this as more or less of an accident, as there were apparently, names of many capable men voted upon; and some of them were dropped through what might be considered a mere technicality, thus narrowing the field to such an extent that my name received more consideration than otherwise would have been the case. Quite a number of people commented afterwards on the fact that Mr. Sprague and I were to be associated on this Board, being about 180° apart on railway electrification matters. However, as we had had no personal differences, and apparently each has had due respect for the other's opinions, we always got along together most harmoniously on the Naval Consulting Board work; in fact, much better than either of us was able to get along with some of the other members of the Board; which all goes to show that engineers can hold radically different opinions on some subjects, and still be very good friends.

EDUCATIONAL WORK

I never liked educational work, but it appears, that, indirectly, I have always been obliged to do more or less of it. While in college, I never did any tutoring, largely because I had no liking for the work, but almost daily the boys would come in to see me about certain matters in their class work, particularly in mathematics. I have said in speaking of my earlier years, that sometimes upper class men would consult me on some of their work;

and that, though I would protest that I was not familiar with the subjects about which they asked me and, therefore, could not be expected to give any opinion worth while, they would tell me that I could make things a little bit clearer than anyone else, and that they would like me to at least to attempt an explanation. At first I considered it an imposition on me to be obliged to help others in this way; but I soon began to see that it was good training for me whatever it might be worth to anyone else.

After I entered the Westinghouse Company, I was more or less forced into the educational field, through the nature of my work. I had to build up methods of analysis and calculation; and, as the work grew, I found it necessary to train others in such matters. In order to obtain the greatest amount of assistance from others, it always appeared to me necessary to educate them in the work just as far as they could go. In consequence, although the process was slow through the early years of my connection with the company, I gradually built up a force of young men to whom I endeavored to impart practically all of my own experiences and methods. Also, as I have said before, I never adopted the policy, too often followed, of declining to have around me men whom I thought might possibly go ahead of me. I have always felt that the stronger I made my particular department, the stronger I, myself, would be; and after all of these years, I have found no reason for changing my notions on this point.

This early work was more or less haphazard, as I "picked up" men as the occasion offered, there being no definite way of selecting such a force. A man that

attracted my attention on account of certain abilities I would go after, and would study his characteristics. As the company grew larger, however, and the need of men of especial ability increased, it was no longer possible to build up the technical forces from the normal ebb and flow of men, for there were not enough good men thus available. Consequently, many years ago, the Westinghouse Company organized a definite Students' Apprenticeship Course, which was more or less of an educational nature. This Course grew to huge dimensions, becoming, in fact, twice as large as it should have been, to meet the needs of the company, adequately. I do not attempt to speak of the success of this Educational Department from all viewpoints, but I will say that at that time it did not at all meet the needs of our Engineering Department. The difficulty was that the students were brought in first, and investigated afterward. When I got into this matter later, I called attention to the fact that the Westinghouse Company should not go into the market blindly for materials and then hope to fit them to our needs after they were brought in. On the contrary, the company should go out and deliberately seek materials suitable for the requirements of our apparatus. I maintained that it was just as important to select the *men* who used the materials as it was to select the materials themselves.

About 1911, owing to certain changes in the organization of the Educational Department, it became necessary for me, personally, to select a number of young men from our Student Course for certain engineering purposes. I went into the matter painstakingly, interviewing practically all of the men then on our Students' Course who had any idea of taking up engineering work. If I remem-

ber rightly, I interviewed something like eighty men. Of this number, probably fifteen to twenty were sure they wanted engineering in some form or other; but, in the last analysis, the whole matter narrowed down to something like five men who appeared to possess anything like the proper characteristics. In view of the fact that we had taken on something like three hundred students that year, this did not look very promising. I contended thereafter that we must stop selecting these men at random, and give proper consideration to the real needs of the case; and that we should then select men to suit these needs. In other words, if our yearly demands in the Engineering Department are for certain numbers of men of certain kinds, we must, as nearly as possible, select a proportional number of men having the characteristics needed, making due allowance for failures and shortcomings. Gradually this policy has been adopted, and has appeared to work out excellently during the past three or four years.

As in recent years, however, it has been found that our needs have been such that the gradual growth of the engineer has been too slow to meet our requirements, I, therefore, also proposed some seven or eight years ago, that I, personally, undertake a special class in the fundamentals of design of electrical machinery. This was agreed to, as an experiment; and after some months of effort, I succeeded in locating a small group of suitable men apparently having the proper aptitudes and characteristics for this work. The first class represented very much hard work on my part, as I had to get down to what I considered the real fundamentals of our electrical work. Practice in designing was not taught in this class,

but methods of analysis, covering the fundamental derivation of most of our practices were taught.

This first class, consisting of five members, did very well. It was planned to organize new classes each following year. However, for the next year or two, it was impossible to find enough suitable men to make up such classes. This showed a weak spot in our educational system; and since then we have given the matter such careful attention that, each year, I have been able to make up at least one class; and in one exceptionally good year, I had enough men to make two classes. As each of these classes is of six months' duration, this has required much work on my part. Possibly the hardest part of the whole business is to select the men for these classes. In some cases, I have taken as long as three or four months trying to find out whether certain of our young men really possessed the proper characteristics for this work.

In addition, we have formed what is called a "General Engineering Class," which familiarizes the Engineering Students with our practices and the reasons therefor. This also is purely educational. In this class, the men spend their entire time for three months or more on this kind of work, and do no direct work for the company during the whole period. This has taken considerable time; but all of our engineers are supposed to take part in this work, so that my share is not out of proportion. As regards the Design Class, referred to above, it is recognized that this is my own private class. However, some of the earlier men from these classes are developing so well that, at times, they are called in to assist with the newer classes.

Some three years ago, the young men who had been in

my Design Classes organized a sort of a "private club" which they named "The Laminations," of which they have elected me the one and only honorary member. I have hinted to them that, in choosing the name of the club, they were apparently trying to make a bad pun on my name; but they have refused to admit that there was any such intention.

CHAPTER IX

PERSONAL TRAITS AND HOBBIES

AS to personal characteristics, side traits and hobbies, nothing much can be said regarding them other than has been indicated in the preceding story. As I have mentioned before, I have a good memory; I have spent much time at analysis of various kinds; I like ordinary mathematics fairly well; and I believe I have considerable persistency. I like to take up new problems and many of these I have worked over, on and off, for many years. If I attempt a problem and cannot satisfy myself regarding it, I can lay it aside and take it up from time to time, as a sort of side issue or change in work.

Some of these problems, as I said, I have had on hand for twenty years and still tackle them occasionally. For the last two years, I have solved to my satisfaction a couple of problems which had been bothering me for more than ten years. I may mention, incidentally, that many years ago, in one of my leisure moments, I made a list of a number of problems and technical matters which I wanted to work out at some future time, there being probably a dozen items in this list. I laid it away and had forgotten it for a number of years. Recently, I came across it again and went over it to see what results I had obtained during these years, and I found that every item on the list, with possibly one exception, had been worked out at some time or other, to my satisfaction. I cite this to show why I believe that I have considerable persistency.

An instance of this came up in connection with my college thesis work, required for graduation. I began this work in the summer before my last year in school, and I got a large part of it out of the way before I had progressed far in my senior year. I desired to save time which I knew I would need on other work during my last year. When I had about completed the thesis, in reviewing it, I discovered a mistake right at the beginning, which invalidated everything I had done. This was a "body blow," and I felt like dropping the matter altogether. However, after a few hours of deliberation, I "turned in" to do the work all over again. This required much effort, and after my disappointment, I simply had to force myself to go on with the work. I felt afterwards that this was a most excellent training for me. The thesis itself did not prove to be of any value, as is practically always the case with theses; but the training resulting from the mistake was certainly well worth while to me.

As said earlier in this story, in connection with my college work, I was not very strong in higher mathematics, but I could use pretty well what I did have. While in school, I liked to tackle practically any problems with which I heard others were struggling. I could not always solve them, but I liked to try them, at least. Some of these problems I worked at, on and off, for some years after I left school. This ability to lay aside a problem and take it up later has been of great value to me in many ways.

One of the many special problems which I have tackled from time to time is that ancient one of "magic squares."[1] At one time, as a recreation, having gone deeply into the

[1] See Appendix. Section "B. G."

construction of these, I evolved eventually certain eight and twelve side magic squares, which, I believe are of the most symmetrical ever constructed, in that not only would the verticals, horizontals and main diagonals add up alike, but each partial diagonal, with its supplementary diagonal on the opposite half, would also add up to the same amount. Moreover, each little elementary *square*, consisting of *any four adjacent numbers* would also add up to a constant value, no matter where such a square occurred in the main square. In other words, there was numerical symmetry throughout the whole arrangement. I first constructed one of these symmetrical squares with eight numbers per side and afterwards built up a 12-square, by analysis from the relation of numbers in the 8-square, obtaining equal symmetry. I then arranged a little game, or puzzle, consisting of small cardboard of black squares, each containing four numbers. The problem was to assemble these to form an 8-square, for instances, so that there would be perfect symmetry in the larger square. I applied for, and obtained, a patent on this, as a puzzle, but no manufacturer was sufficiently interested to bother with it, so that it represented money wasted.

As to some of my side traits, I have always been fond of reading, but not usually of the so-called "solid" type, except in the way of ancient history, or archaeology. I have always read, more or less, for recreation, choosing the type of literature which furnished most recreation. I found, when I was a boy, that something of a highly imaginative nature appealed most to me. I did not care for what were called "blood and thunder" stories, as they did not show any particular evidences of real imagination. As I got farther along, semi-scientific stories of the most

imaginative type appealed to me as best of all; and this taste seems to grow stronger with time. Apparently, it is the imaginative that has appealed to me in almost everything, as indicated by some other traits which have been predominant in my case.

Apparently, this is back of a taste for archaeology and the study of antiquities, which is quite strong with me. This trait, apparently, began when I was very young, but I did not recognize it as such for many years. The first evidence that ever came up in this regard was when I was about four years of age. I have only a faint recollection of the matter myself, but the incident has been related to me by my elders. It appears that when I was about four, one day, while riding on a "roller" over a field which was being prepared for cultivation, I saw a queer "stone" in which I was much interested, but which I could not gather at the time because I could not get off the roller. The next day, having missed me for some time, they later discovered me, trudging in from a distant part of the field where I had noticed this stone. I had found it and was bringing it home. It proved to be a stone implement such as those fashioned by the earlier Indians. A year or two later, I remember distinctly that, when out in a field along a little gutter formed by a recent heavy rain, I began to search over the exposed pebbles to find something manufactured by the Indians; I succeeded in finding a flint arrowhead. Although very young, I evidently had some definite notion in mind regarding the collection of such things.

In the following years, both when a small boy, and when I grew up, this interest in such things grew even stronger, and it is very strong, even today. I made quite a large

collection of stone implements, arrowheads and knives, mostly found by me. However, as I knew of others who had a somewhat similar interest in such matter, I assumed for many years that it was nothing out of the usual. In my reading, I was also particularly interested in archaeology and antiquarian matters, but this did not indicate anything in particular to me. It was only some years later, while traveling through Europe, that I began to appreciate that there was, in my case, a strong leaning toward archaeological subjects. I found, in these travels abroad, that I was not greatly interested in modern buildings and works. I did have an interest in beautiful scenery, as a whole. It was when I reached Zurich, however, that I had the first strong evidence of anything that attracted me unusually. Happening to visit the National Museum at Zurich, I saw the fine exhibit of the implements and weapons, of the prehistoric lake dwellers. This appealed to me extremely, and after going through the museum I would return often to this part of the exhibit. It had an attraction for me far above anything which I had seen in Europe up to that time.

As I traveled down into Italy, a strong interest in the older buildings and works began to grow. However, I did not feel the full strength of this appeal until I reached Rome. Soon after arriving, I started out on foot, with my brother, to look up the Roman Forum. We got lost on the way and in wandering around, we came upon the Forum quite unexpectedly at a point where we got a most complete view. My first momentary feeling was of extreme anger at the wanton destruction of the place; but, of course, almost instantly, I remembered that this had happened through a long period of time. However,

there was an attraction here that was exceedingly strong, not only for the Forum but for all the other ancient works about the city, and in the museums of antiquities. I returned to the Forum and other ruins time after time, and could hardly drag myself away. Apparently, modern and medieval works, like St. Peter's, did not have this attraction, although I visited many of them. I watched some workmen digging in one of the ancient palaces on the Palatine, and I felt that I could stay there and watch them indefinitely.

This appeal was apparently much stronger than any I have ever felt for anything else, including engineering work. Since then, I have gone into the matter more fully with a view to finding what of this nature does attract me. I have had for years a very great desire to visit Sicily, and I find that it is the old Greek temples and ruins there which attract me more than anything else. I have also desired to visit Greece on account of the ancient Greek works. Egypt, too, has a great attraction, but only as regards the very ancient works. Palestine and India have no temptations, for some reason or other, but I have longed to go to Bagdad, apparently because of its proximity to old Babylon. Obviously, it is purely the archaeological and antiquarian inclination which is back of it all.

I have tried to find a connection between this and my liking for engineering work, and for imaginative reading. It seems to me that it is the imaginative element in them that interests me mostly. When I consider various other traits, and likes and dislikes, it appears to me that the imaginative features are possibly at the bottom of it all. On reading a highly imaginative story, I usually visualize

almost everything as I go along, and apparently do not even see the written words; at least, afterwards, often I have very little recollection of them. In other kinds of literature where I cannot use such visualization I usually soon lose interest. In archaeological lines, I can picture most of what I am reading, although doubtless most of my visualizations are incorrect. In all such visualization, I am only present as an observer, in fact, I almost never form any part in what I "see." As regards engineering work, especially machinery, I can usually picture, crudely, in my mind, the thing I am after, and can visualize its internal actions, to a considerable extent. Thus, the physical conception is apparently much stronger than the mathematical conception, in my methods of analysis. I even carry the former into my mathematical attempts, and I suspect that this is one of my great difficulties in mathematics, as it probably is with many people.

Summing up the whole matter, it seems that imagination is a rather predominant trait with me, and that I have, both consciously and unconsciously, encouraged its development. In later years, this encouragement has been conscious, as I have recognized its advantages. In my childhood, it was a great source of pleasure, but I almost never said anything about it; or, I may say, I hid it from others, because there was a tendency to discourage such things, as is too often the case.

As to habits, the late Mr. Walter Kerr of Westinghouse, Church, Kerr and Company, once told me, "you must kill sheep, as you don't seem to have any other bad habits." I have never smoked, and as far as I remember, I have never cared to do so; in fact, it has always

seemed such a useless thing to do. Also, I have never used liquors in any form and have never desired to do so; in fact, it has always seemed such a disgusting thing. I never use profane language, as it adds nothing to the strength of the English language. Chewing tobacco is abhorrent and always has been. I am considered mild-tempered, that is, I do not become irritated or angered easily. I do not worry and have never done so; it has always seemed to be useless, and really harmful. If I find a tendency to worry about some matter, quite frequently I look up one of my old-time unsolved problems, go to work on it and forget the worry. In fact, I found, long ago, that I could voluntarily drop any subject and take up another, at will.

Often I have turned this trait to good advantage. For instance, many years ago, I used to suffer, at times, from facial neuralgia, principally after retiring at night. The more I thought about it, the worse it got. Obviously, I must reverse the action. So I would deliberately take up some long, difficult problem or design, requiring much calculation; in many cases I put myself to sleep without any difficulty. Some people might intimate that this was Christian Science, but I take issue with that; for I think there is a simple physical explanation for the disappearance of my neuralgia under the circumstances.

I do not believe in fads and fool notions. I have no superstitions, as far as I can determine. In fact, when I was half-grown, I deliberately put to the test a number of the superstitions of my neighbors and associates, regarding plant growth, weather prognostications and such things; and I soon came to the conclusion that they were pure nonsense. However, in discussing the facts with

some of the older folk, I soon found that most of them did not want their pet superstitions upset.

As to hobbies, I suppose I have had some, in a minor way, ever since childhood. The collection of Indian stones, as mentioned before, might be considered as a hobby. I was never interested in collecting stamps, birds' eggs, or anything of that sort. They did not seem to me to have any real value. I never care to work with flowers or do garden work, so that I have no hobbies of such kind.

In later years, I took up photography as an amusement, and somewhat later began to experiment with telephotography. My first outfit I made myself, using one barrel of a pair of Lemaire opera glasses. Getting pretty good results with this, I afterward rigged up one barrel of a pair of Warner-Swasey eight-power binoculars, and got pretty good results with an equivalent focal length of about 90 inches. I then purchased a telephoto tube and lens from one of the regular lens manufacturers, but could not get as good results as with the opera glass outfit. Later, I bought a larger and more powerful telephoto lens and tube, of a different make from the former one, with somewhat better results. Still later, I made various combinations of the lenses of the different telephoto outfits by having special adapters made, and found that with the double concave negative from one outfit and the positive of another, I could get most excellent pictures with a focal length equivalent to nearly 100 inches.

The idea back of this telephotographic work resulted from experiences which I had in Switzerland and Italy. I found that certain most magnificent landscape views were at such distance that the ordinary short focus camera

did not show them to any advantage and that when I took the trouble to get up close to these views, they were entirely spoiled. It then occurred to me that if I could only so arrange my camera that I could take these distant views on a much enlarged scale, I would have what I wanted; so I spent a part of my spare time, for several years, experimenting with my different telephoto outfits, until I found something which seemed to be practicable and satisfactory to myself. I thus got ready for taking long-range views in case I should ever get to take my long-desired trip to Sicily and lower Italy. But as there has never been an opportunity to make the trip, my outfit has never really been used, except in an experimental way. I believe I have one of the most compact, easily portable, quick focusing, long-range telephoto outfits that has ever been assembled. The principal difficulty is that, for obvious reasons, it can seldom be used in Pittsburgh, where I live.

I do not know whether this last interest could be called a hobby. It seems to me to be more in the nature of an experimental development. As to ordinary photography, I usually take a camera with me when I go on my vacation, once a year, but use it more to make a record of the yearly changes occurring in certain neighborhoods, than for anything else. For example, I have a picture record of all the changes of importance which have occurred in my old home in Ohio, during the past eighteen years. I have done considerable enlarging of photographs, but as this was more of a burden than anything else, it could hardly be called a hobby. I never did anything of the nature of color photography, for that always seemed to involve an extraordinary amount of hard work.

CHAPTER X

OLD TIMERS

IN connection with our Garrison Alley Works, I have already mentioned a number of my associates, principally those with me in the testing room, and members of the Laboratory. However, there are a number of men, whose names are well known, with whom I came in contact from time to time.

Throughout this story, I have made a number of references to Mr. George Westinghouse. I do not remember when I first became acquainted with him, although I did see him quite often soon after I entered the test room, in 1889. He had a steam turbine (called a "rotary engine") with which he was experimenting. It was coupled to a 133-cycle alternator of standard make. This outfit was operated from time to time in connection with our shop lighting circuit. The test room, of course, had to take care of this unit, and thus, I very often saw Mr. Westinghouse. Later, in our test room work, I had considerable to do with him, from time to time, in connection with the development of apparatus; but I cannot recall just when he knew me as an individual rather than a member of the testing force. When we took up the street railway equipment work, he used to talk to me about the apparatus occasionally; and when we built the first single reduction equipment, he apparently knew me quite well.

Around the shop and testing room, he was quite pleas-

ant in dealing with the men, but never unduly familiar. In fact, most of us were more or less afraid of him at first, as he could be quite "sharp-spoken" when he thought things were not being done right. In all my relations with him, through many years, I really never was in close contact with him, except in engineering matters. In such, he used to talk with me very fully, and sometimes quite confidentially, regarding his general ideas. On other subjects, he seldom talked to me at all. On the turbo-generator work, when it began really to get started, I came into closer contact with him than on any of the earlier work, as this was one of his "pets." As soon, however, as this work was put into reasonably good shape, he took up something else.

Mr. Westinghouse seemed particularly attracted by men of large stature, apparently taking such people into his confidence much more readily than men of smaller build. I do not know whether this made much difference, in the long run; but apparently some of us "smaller built" men had to attract his attention almost entirely through our work, rather than our personalities. Also, if a man talked "big" enough, he would attract Mr. Westinghouse, to a certain extent *at first;* but if he could not live up to his talk, Mr. Westinghouse would soon drop him. He was a far-seeing man, and, in considering any engineering problem, if the subject appeared to contain very large future possibilities, he did not "stagger" at immediate difficulties. I remember on that one occasion, in connection with some special apparatus which we were contemplating, he authorized the taking of an important contract embodying this apparatus, and then said to me, "Now, I have thrown you into the middle of

George Westinghouse.

the pond and it is up to you to swim out." He believed, apparently, that most engineers did their best work when hardest pressed; and no doubt, to a certain extent, he was right. In the last few years of his life, after his financial troubles of 1907 and 1908 he did not come to the Westinghouse Electric works very often, and when he did so, his mind seemed to be very largely on other things than the Electric Company's engineering. In consequence, I had but few conversations with him in his later years, and these were more or less irregular or disconnected.

Mr. William Stanley was intimately associated with the beginning of the Westinghouse Company, but at the time when I entered it, he apparently had but little working connection with the company, as he came to our works only at rare intervals. I saw much of him when he brought out the alternating arc system in the latter part of 1889 and 1890. Apparently, it was believed at that time that the constant current alternator, as it was first brought out by the company in connection with Mr. Stanley's arc system, was a very special type of machine. However, when I took over this work, in taking charge of the arc testing room, which I have spoken of previously, I did much work on this constant current alternating system and developed, eventually, a self-exciting machine. At that time, Mr. Stanley insisted that his arc alternator was a machine of very peculiar properties; but after I got into the work, I told Mr. Schmid that it was simply a constant potential alternator worked so far out on its regulation curve that its e.m.f. regulation was extremely poor. In this arc work, I probably met Mr. Stanley a number of times, but I did not think that he ever knew

me. I met him two or three years later at Pittsfield, in connection with some railway work, and he then apparently remembered me. However, many years later, I used to meet him at conventions, and he used to speak of the old times at Garrison Alley as though he remembered me very well, so it is probable that we were really better acquainted in the earlier days than I thought. I remember Mr. Stanley particularly on account of his quite nervous disposition. I liked to hear his stories of the very early development, around 1886, when the company was just beginning. Some of the experiences he told me were very amusing, from the present viewpoint, but in those early times, there was nothing so funny about them. Mr. Stanley was particularly fond of telling stories *on* Mr. Guido Panteleoni, who was associated with him in the very earliest work of the Westinghouse Company.

I do not remember just when I first met Mr. Panteleoni, but I had heard of him from the beginning of my connection with the Company. As he often came to Pittsburgh from St. Louis, where he was located, I undoubtedly met him at various times in my testing room work. In fact, in my earliest clear recollection of knowing him, personally, I felt quite well acquainted with him. In my early experience with him, he always appealed to me as a man with very positive views, which he could reverse on a moment's notice. In consequence, he was a hard man to "corner" in an argument. In my later acquaintance with him, he would not hesitate to say that he had no confidence whatever in our engineering; and yet, at the same time, he was willing to sell anything, without question, if we told him it was all right. For instance, Mr. Panteleoni sold two comparatively

large induction regulators some twenty-two years ago, when such things were quite new. He then told me what he had done and asked me, quite excitedly, to explain to him what it was he had sold.

When he got properly started, he was a great hand at telling stories regarding the early developments of the company, particularly with reference to Mr. Stanley. I used to take Mr. Stanley's stories as a basis to get Mr. Panteleoni started to tell the same incidents from his viewpoint; and I found great amusement in comparing the two stories afterwards. Fundamentally, both stories would agree pretty well, but the two viewpoints were radically different. In embellishing these talks, Mr. Panteleoni was the greater artist of the two. Some of the boys would say that he got pretty badly mixed in some of the facts in his narratives, but, in general, I found that there was a pretty good foundation for many of his statements.

Fred Scheffler was made Superintendent in 1890; and for some reason or other, I got well acquainted with him in a comparatively short time. He used to come around the test room and feel the machines to see how hot they were. On one occasion, he felt a frame of a Waterhouse arc machine, which was running on an insulated base, and got a pretty heavy "jolt." He asked me what was the matter and I showed him that there was a very heavy "static" on the belt, saying that possibly, as the machine was well insulated from ground, he had got a shock from that cause. He said he supposed that static was not really dangerous. I told him that, of course, it was not, but that it was better for him not to put his hands on the machines. After he had gone, I asked one of the

machine tenders about that particular machine. He said that it was dead grounded through one of the arc lamp racks, and that Mr. Scheffler had got a pretty severe shock. He said he knew, because he had got the same "dose" himself.

Another man, who came to the company in the early days, was H. McL. Harding, who took charge of the railway sales work early in 1890. Mr. Harding, although dressed "spick and span" would persist in coming to the railway test room to find out what we were doing, and I, therefore, became extremely well acquainted with him there. He used to ask innumerable questions about things, but I never could see what he wanted to do with most of the information.

I must not forget Nikola Tesla, who was one of the "Old Timers." When I first came into the company, in 1889, Mr. Tesla was still actively engaged on the development of his motor. They were even considering it for street car propulsion. A short section of track had been laid in a big lot next to our works for the purpose of making some tests. I do not know, however, whether the matter ever got as far as building a motor suitable for mounting on a car. Mr. Tesla was a fine fellow, especially around the test room. He used to say that he furnished, *involuntarily*, high grade knives to nearly everybody in the test room and machine shop. He always bought good pocket-knives, costing about $2.50 and everybody soon learned this. He always said that if he happened to lay his knife down, and turned his back for a moment, he was "out" a pocket-knife. I undoubtedly met Mr. Tesla very soon after I entered the test room, as C. F. Scott was Mr. Tesla's assistant, and he, no doubt,

introduced me. I used to watch the induction motor work when I had the opportunity but I could not make much of it as, at that time, everything was "cut and try" and I did not have any knowledge of what they were trying to do. However, I became much better acquainted with Mr. Tesla after he moved to New York a few years later, and he used to give me most interesting information and advice. The last time I saw him, a number of years ago, he happened to drop in unexpectedly at our East Pittsburgh works at lunch time. He came to the lunch room alone and looked in at the door. There was a large table full of people and I was the only person in the room that had ever met, or had even seen Mr. Tesla. As soon as I saw him, I invited him to come in, and motioned him to the only vacant place at the table. I did not take the trouble to introduce him to the people individually, but simply said, "Gentlemen, this is Nikola Tesla." He then sat down, and in a few minutes, through his conversational powers, he was dominating the whole tableful of people, and talking to everybody as if they were old friends.

Among the "Old Timers," in the testing room, was Cal. Humphrey, who later became head of our Supply Department. We both entered the testing room about the same time, that is, in the spring of 1889, and both had very much the same work. He and I used to have great times together, having friendly quarrels with each other, and "knocking" everything under the sun. Apparently, however, judging from his talk, he had more grievances than I had. Finally, after a few months, he was sent on some "outside" work, and as usual, I felt that I was one of the "left-overs."

Others in the early test work were Frank N. Waterman, now the well-known patent authority; Archbold, of Archbold and Brady; E. W. T. Gray, late manager of our New York Office and still later in the insurance and banking business; and Th. Gonet (the "Frenchman" as we called him) with whom I was very closely associated in my early work. Mr. Gonet could not use English very well, and not infrequently I would correct him, always explaining the meaning of my corrections. Finally, one day he thanked me and said that I was the only man that he knew who tried to help him learn the language, as everybody else laughed at his mistakes. This was the first evidence I ever had that he was at all sensitive.

There are many other "Old Timers," not all of whom I can recall at the moment. There was Gates, of Chicago, and Bragg, of our Philadelphia Office, both of whom I used to see often; and both of them are gone these many years; Maurice Coster, who is still with us; Sanderson, of our Boston Office, now of Sanderson and Porter; Townley, who is still with us; C. S. Cook who has gone to the Duquesne Light Company; W. F. Zimmerman, formerly head of our New York Office; Frank Smith, of the Lamp Company, and many others whom I used to see very often. There was Blunt, later with the British Westinghouse Company; E. C. Means, head of the Draughting Room under Mr. Schmid, with whom I was particularly well acquainted; L. A. Osborne, Vice-President of the company, who was principal engineer of the United States Company prior to the time that its engineering work, with its engineering force, including Mr. Osborne, was transferred to East Pittsburgh, in 1895. There was Oscar Baldwin, who went to England as rep-

resentative of the company; and I must not forget C. A Terry, representing the legal and patent interest of the company in those old times, very much as he does today.

I became acquainted with Mr. Terry very early in my career, through patent applications, and he was extremely kind and helpful. One of my early experiences, in which he took an indirect part, I shall always remember with quiet amusement. In the latter part of 1892, the question came up, in some way, regarding compensation for some of my patents. There had been no agreement in these matters when I came into the company; and by 1892, some of my inventions having become of value, several of my friends thought the company should pay a little something to me. The matter was therefore, referred to Mr. Lemuel Bannister, at that time Manager of the Company. He called me into his office to take the matter up, and had Mr. Terry present, on account of the patent questions involved. Mr. Bannister wanted to know what compensation I expected. I named, off hand, a certain sum which, in view of my past salary and the number of patents which I had applied for, was not at all excessive. Mr. Bannister then began to discuss the matter. He said among other things that it was not known that the patents involved would be of any value, and that the company would be buying them on chance. I informed him, however, that these patents were largely on devices which had already been adopted, such as the machine-wound coils on the No. 3 railway motor, and that we had built something like a thousand of these motors. That "stumped" him at first, but he came back with the statement that it was sometimes the practice to put a man under a contract for a specified time,

the agreement covering all back work in the way of patents. He stated that the price I had named was entirely prohibitive, although I had already said that the lump sum could include my salary up to a certain date. I then said I would consider a contract, provided that they would give me a small cash payment to cover the patents, in addition to the contract salary for a salary period of about three years. He then asked me to name my terms for such an arrangement. Being pretty good at figures, I named, almost instantly, a small cash payment and monthly salary, in addition, for the whole period, asking him whether that would be satisfactory. He turned to Mr. Terry, who was beginning to figure it out on paper. I watched Mr. Terry very anxiously, because the small cash payment, plus the salary for the period totaled just exactly the amount I had originally named, and I knew that he would soon find that out. He figured away for a little while, and when he made his final summation, I knew from his expression that he had "tumbled" to the fact that my two offers came to the same figure. However, when Mr. Bannister asked him what he thought of my last offer, he replied at once that he thought it was entirely satisfactory; but he didn't name any totals. Of course, after that, I had a still warmer regard for him, for I felt that he would stand by "us boys" when the opportunity offered. I never mentioned this incident to Mr. Terry, in all the following years and probably he forgot it soon; but I did not, as I had such excellent reasons for remembering it.

There were many other "Old Timers," some of whom I did not know as well as those I have named. In the shop, many men whom I knew very well in those days

are still with the company, while others have been gone for many years. There were Nolan, Baird, DeKaiser, Bell, and I must not overlook Herman Boegel; he it was who telephoned from the Allegheny works to the Garrison Alley storeroom, "Send over four bolts just like I have in my hand, what?" Herman's specialty, at one time, was engine type alternators, and they were special in the fact that usually they rocked sidewise, in time with the rotation of the engine. In due course of time, Herman left us and went to Europe obtaining a position somewhere in Belgium. Later, at the Paris Exposition, in 1900, as I was walking down the machinery aisle, where a number of large engine type alternators of various makes were in operation, I turned to Edgar Reed, of our French Westinghouse Company, and said, "I see an alternator down at the far end of the aisle that looks like Herman's, as it is rocking sidewise." He said, jokingly, "Maybe it is Herman's, as I understand he is Superintendent of a factory over in Belgium"; and he named the Company. A few minutes later when we came to this "rocking" machine, we looked at the nameplate and it was that of Herman's company. Apparently nobody ever discovered the secret of how he accomplished this peculiar action of his machine; for none of our machines before or since have ever possessed that particular trait.

Throughout the offices at Garrison Alley were a number of men with whom I became acquainted in the early days who have "stuck" to the company through "thick and thin"; such are W. J. Longmore, for many years Purchasing Agent for the Company; J. C. Bennett, now Comptroller; Herman Baetz, now Treasurer, and many others.

In the Engineering Department, I have already mentioned most of the very early men, and therefore, nothing further need be said.

After the company moved to East Pittsburgh in 1894–5, it grew very rapidly; and the number of men that I have been in contact with has been so great that it is useless to attempt a list. Quite a number of the leading men in the company have developed since that time. Consequently, in referring to "Old Timers," I have, in general recalled only those corresponding to the Garrison Alley period.

The above covers practically the first thirty years of my work and association with the Westinghouse Company. It is not a very complete story, as there are many lines of effort that I have taken up from time to time, which have not led to any particular results and are not herein mentioned. I have endeavored to touch the "high spots" only. There have been many interesting incidents in this work which I have not given; these I may desire to fill in at some future time.

APPENDIX

"B. G."

THE foregoing narrative was begun by Mr. Lamme as a record of the first thirty years of his engineering practice and it was not until several years later, near the end of his life, that he expressed a willingness to have it serve as an autobiography. This explains why the material is primarily an engineering history rather than a broadly human account of his life.

In this connection it must be borne in mind that to Mr. Lamme his work *was* his life. The amount of work done outside of regular office hours, and the nature and purpose of that work, bespeak the loving interest which only an artist puts into the thing which he is creating. But he had to a marked degree an interest in people and a sympathy for their problems arising from human sources which made his personal friends feel for him a tie of affection aside from their esteem and admiration for him as a great engineer. The title of this chapter (the first two initials of his name) was the almost universal nickname by which he was known among his associates and is the witness of the hold he had upon the hearts of those who knew him long and intimately. The warmth of the affection he aroused urges that the record of his life take account of the breadth of his humanity as well as the brilliance of his work. This is the apology for this appendix which sets forth some incidents of his life bring-

ing out sides of his character too briefly touched upon by his own hand.

As a boy he seems to have been a sane, healthy, out-of-doors youngster who was interested in any and all things that touched his life. The photograph of him taken at four years of age indicates a wide-awake normal American farmer boy to whom we can readily believe all forms of life and all boyish pastimes were interesting.

One of Mr. Lamme's theories was that in a born engineer the "constructive tendency" appeared early in life and his own experience bore this out. His father had the usual farm work shop equipped with wood working and other tools and the boy spent much of his time making his own toys and probably getting fully as much fun out of the planning and building as he did from putting them into operation when they were complete. His brother remembers a very substantial wheelbarrow of that period and a wagon so well built that twenty years later parts of them were still in existence. This same care and thoroughness went into the design of motors and generators, many of which are no doubt running today and fulfilling the promise of that boyhood wagon and wheelbarrow. There are stories, too, of an old grandfather's clock in the attic which fascinated him and which he tried to put into operation but failed, owing to missing parts. He then took the various wheels and rigged them into a gear train with the result that turning the first wheel by hand caused an excessively high speed of rotation on the last one, to his great delight. He mentions the same pleasure in his own story of the experiment with the wheelbarrow and grindstone and spool at a later date. One of the interesting sidelights to the story of the clock experiments is

Portrait in 1920.

that a sister sat and watched him at his work and no doubt shared with him the hereditary desire to "make things and make them work."

Another incident, pleasing in reminiscence, because it illustrates that he was a real boy as well as a real engineer, is the story of a water wheel. The road from the old homestead turned into a lane along which ran a brook fed by springs and hence having a fairly even flow. "B. G." made a chute and placed it in the brook above a waterfall about two feet high. Beneath this chute he installed a handmade overshot water wheel which ran beautifully but not being harnessed to any work, failed to make the noise which he felt it should, to advertise its virtues to the world. So, to add to the noise, he placed a cam on the shaft which raised a lever with a large nail in the outer end. With each revolution of the wheel this nail was raised and let fall upon a properly placed tin can. The audible results were eminently satisfactory to the builder, but were also too much for the nervous systems of passing horses and in deference to numerous complaints lodged with his father by passersby, this portion of the hydraulic installation had to be eliminated. It is a pleasant and very human touch presented by the boy who had accomplished his engineering one-hundred per-cent, but who was not satisfied until it produced enough of something to indicate that it was really working.

All the stories which are told of Mr. Lamme are characteristic of some one or other of his traits but they agree in bringing out forcibly the fact that he was at all times a most normal and sane man. He had various hobbies and special interests at different times and he followed them enthusiastically and whole-heartedly, but

he never went to the extreme that sometimes marks a character less broadly based and carefully balanced.

Two of his hobbies are mentioned elsewhere in this book, viz.: photography and automobile driving. A third on which he spent considerable interest was in making and solving different kinds of puzzles. In this appendix[1] is a reproduction of one of his patents on "magic squares," a mathematical form of puzzle at which he was adept. He was also very fond of a form of puzzle called Richter puzzles, which consists of making a large number of combination designs from sets of small terra cotta blocks of various forms. He took great interest and pleasure in these and bought all the different sets he could find for his own amusement and as gifts to his friends.

Perhaps the best sketch which can be made of any man is a composite of the little intimate touches which only his close associates see and remember but never appreciate fully until after he is gone. One of his characteristics was that he had an endless capacity for hard and strenuous work and never spared himself when he felt that such work was really an investment and the results of it might later appear as a service to many others of his fellows. But when that hard grinding toil seemed to be only an end in itself, it did not attract him. That was the reason why the drudgery of the daily routine of farm work in his day was distasteful to him, but, on the other hand, he would school himself to get along on five hours' sleep a night in order that he might improve his mind and work out engineering problems which paved the way for his later achievements. Many stories are told of his

[1] See page 252.

habits of industry and of his capacity for concentrating all his energy on the immediate problem, no matter what distractions might surround him. A very interesting story bringing out this point is told by a brother-in-law who visited him in Pittsburgh in 1894 at a period when he was in the midst of the growing activities which he describes in his own story. Mr. Lamme was very fond of the theater and to entertain his guest took him to the performance of a musical extravaganza with Anna Held in the title rôle. This was altogether a very lively and pleasing entertainment but his guest reminiscing across the years says: "We went to the show, but I observed that all during the performance he was figuring on his shirt cuffs. He conversed with me about various matters during the intermissions and I was rather amazed at the close when he asked me if I thought the show was any good. Apparently he had seen and heard little of it but was concentrating on some mathematical problem." This must, indeed, have been a hard evening for Mr. Lamme, because the young men who associated with him in those days say that he enjoyed the theater so much and they attended so regularly with him that a certain row in the old Duquesne theater was known as "Westinghouse row."

Another incident brings out his keen observation at all times and his interest in putting old things to new uses where they might prove an advance over known practice. It was his habit every fall to pay a visit to his sister in Springfield, Ohio, and to spend some time squirrel hunting in the woodlands on the old homestead about nine miles distant. He never hunted with a shotgun, but used a Winchester 25–20 rifle. He was an excellent shot

but never killed more squirrels than could be eaten for one meal. On these expeditions he was often accompanied by his brother-in-law, who gives us this delightful picture of the busy engineer on his holiday. "It was our habit during these hunting expeditions to take our lunch along and eat it in the woods. Through this intimate association I came to know Mr. Lamme as men are not often known to each other. His conversation was of so substantial a character that anyone who listened was enriched by the experience. Even in his recreation he seemed to have some fixed and definite purpose in everything that he did. We were passing a thorn tree one day in the woods when he stopped and cut off a number of thorns. I had no idea what his purpose was until during the evening, after our return home, I found him experimenting with the thorns as needles in the phonograph. I have the impression that it was more or less of a humorous experiment, yet the habit of investigation evidently was so firmly fixed that he could satisfy it even when bent on the pursuit of pleasure."

The reference to his ability in rifle shooting is suggestive of another trait which was remarked by many of his associates. This was his willingness to follow any hobby or pastime to considerable lengths so long as he felt he could excel in it, but to drop it very quickly as soon as he had satisfied himself that through natural aptitude or training others could outplay him at that particular game. This was also true of bowling. At both of these sports he did very well and could, in fact, make an excellent showing. However, as soon as he had established the fact that his own maximum performance was something less than the possible record, he quickly discarded

these forms of recreation for others. This result is by no means unique since this is the usual reaction upon the ordinary man. But in his case the underlying motive seemed to be not merely loss of interest or pique that some one could beat him, but a sense that he must keep on exploring life in search of the things that he could do better than anyone else and leaving to others the thing which they could do best. He had a feeling that the creator had put within him the capacity to do certain things very well and it interested him greatly to find what these things were and to perfect himself in their exercise. This was a very interesting manifestation of his psychology and if the same theory were practiced generally, no doubt the result would be an increase in social efficiency.

As he states in his manuscript, he had a wide experience in judging the capabilities of younger engineers and in helping them find the work for which each was best suited. In following this work he developed a technique one of whose points was that the "constructive tendency" which lies back of most conspicuous designing engineering talent was present in early boyhood. Following this idea he sometimes studied children to see if he could establish any pronounced proclivity. Of one of his nephews, the boy's father tells this story: "I remember quite distinctly when my son was about ten years old Mr. Lamme gave him a very fine set of mechanical toys. We, of the family, thought this was merely a characteristic generous act of the boy's uncle. However, he observed the lad narrowly at his play with the toys and when, upon the occasion of a later visit, he found that the youngster had tired of the toys, he remarked quietly, 'He will never make an engineer.' This prediction came true and indi-

cated to me that in this instance his method of analysis was entirely sound."

Another trait which was quite marked was a pronounced inertia in the matter of taking up certain new things or ideas but coupled with a full and complete acceptance of the new condition after he had finally satisfied himself and made the necessary mental adjustment. An instance of this was in connection with Mr. Lamme's purchase of his first automobile. As was the case with some other things, Mr. Lamme had been for several years twitted by his associates because he did not purchase and ride in a motor car. (It appears later from his sister's statement that this was in large part due to the fact that in childhood it caused him physical discomfort to ride in any kind of a vehicle.) This situation had existed for several years when it happened that one of the Westinghouse engineers concerned with electrical equipment for automobiles paid a visit to an automobile show at Motor Square Garden in East Liberty. He had selected an hour between 5.30 P.M. and 7.00 P.M. seeking to avoid the crowds while making necessary notes on the details in which he was interested. Suddenly, to his dismay, he discovered Mr. Lamme not far away and knowing his alleged disinterest in motor cars, and at the same time his absorbing interest in all things engineering, assumed that Mr. Lamme also had planned to come at a quiet time when he could look at the motor cars without being joked about it. Accordingly, our engineer proceeded to efface himself from the picture, but not before Mr. Lamme discovered him and in the most cordial way inquired, "Have you looked them over yet?" On receiving a negative answer, Mr. Lamme invited the other man to join

him and the two examined and inspected busily for an hour or more. But, curiously enough, Mr. Lamme's great interest was in colors and lines and upholstery and comfort of seating and practically none on engines or carburetors or other mechanical details. He said, "Those parts were made by experts and should be serviced by experts, so why need I bother with them?" Shortly after, he bought his first car and so complete was his change of attitude that he drove it 32,000 miles in two years! Also, he boasted that he had never even looked under the hood!

Mr. Lamme's sister Bertha, later Mrs. R. S. Feicht, stood in a unique and intimate relation to him in that having grown up with him as sister and playmate, she also followed him into the profession of designing electrical machines and worked with him while achieving marked success of her own in the same art. Because of her unusual opportunities of knowing her brother and of judging his work, she has prepared the following notes which bring out points of his personality that impressed her.

"When my brother became interested in anything which appealed to him strongly, as he advanced in it he pursued it enthusiastically. He speaks of his photographic work. A trip to Europe caused him first to buy a camera and he followed up photography through its various stages as long as he found anything to interest him. On his last trip to Egypt and other countries on the Mediterranean, he took fifteen hundred pictures. While some of his interest was in the scientific side of the subject, a great deal was in love of a beautiful picture. He much enjoyed a visit to an art gallery and was particularly fond of a good water color.

"Another hobby, which he took up in his later years, was driving an automobile. As a child he would not ride in a carriage if it could be avoided, as it made him sick. He would walk for miles rather than ride. Probably for that reason he was slow in getting a car. As with photography, an incentive was all that was needed. In this case it was the serious illness of a sister and the car was purchased partly to take her riding. But he became so delighted with it that he drove whenever the weather and his duties permitted. He soon learned all the good roads for miles around as well as all the by-ways. It was recreation to him and he never seemed tired even after a ride from Pittsburgh to Philadelphia or Washington in one day, with no relief from driving.

"In contradiction to the old idea that a mathematician has no music in his soul, he was very fond of good music. When the Regina music box came out years ago, some of our first 'canned' music, he had one of the first instruments and haunted the supply store for new records. When the phonograph supplanted the Regina he was an early enthusiast and had a large library of records, the selections being almost altogether classical music, as rendered by grand opera stars. His music memory was as keen as his memory for other things and on listening to any one of several hundred records in his collection, he could immediately recall the composer, the opera, the selection, and the artist rendering it. For quite a period he spent much of his scant leisure time listening to new records and adding to his collection those which interested him.

"Mr. Lamme touches a little on his reading. When we were children whenever he told me a book was good

and advised my reading it, I always did so and enjoyed it thoroughly. In this way I read much that was old for me, but it was a great pleasure to me nevertheless. So much has been said about his liking for weird stories, detective tales and like literature,[1] that many may think that he cared for no other kind, but such was not the case. He was all his life an inveterate reader. He liked fiction, especially an imaginative or historical novel. He was very fond of history and had many volumes of history and travel as well as sets of standard fiction in his library. He bought all the good fiction magazines to be had. His wide reading and wonderful memory made him an unusually well informed man.

"I have been asked to tell of his influence in leading me into an engineering course for my college work. I cannot recall that he ever directly urged me to study along such lines, but he must, by suggestion and his own enthusiasm for such work, have led my mind in that direction. I had no aptitude for mechanics or doing things such as he had as a child, but I did have a liking for mathematics. For this reason he probably thought I would succeed in engineering. I remember at one time he told me very earnestly that when I had finished we would take up together the business of designing mechanical toys—that it was a "big field." But before I was through school, he had undoubtedly found his "big field" in the electrical business. Owing to severe illness early in my sophomore year, I missed the rest of that year in school. In the spring I visited him in Allegheny and he coached me in some subjects so that with a little extra work, I was able to graduate with my class.

[1] See Appendix, p. 229, for list of these books.

"He had a decided aversion for public dinners and banquets and seldom attended one if he could avoid it, especially if there was a chance he might be expected to speak. Shortly before he was to be presented with the Sullivant medal at the Ohio State University,[1] a sister remarked to him that in a few days he would be highly honored. He said, 'Yes, and I'm enjoying it about as much as the early Christians enjoyed the prospect of being fed to the lions.'

"A trait of his which is perhaps rather unusual is that he did not believe in working primarily for the money reward involved. He has remarked in my presence on more than one occasion that one worked to accomplish something which would benefit the world and that if something was accomplished, the reward would come. I think he tried to instil this into the younger engineers with whom he was associated.

"Although quiet and reticent, he was very fond of his friends and fellow workers. He had a very deep liking for most of the young engineers with whom he came in contact, as well as for the older men with whom he had grown up in the business.

"In closing, I should like to speak of the high mutual regard which existed between my brother and Mr. George Westinghouse. Many times B. G. spoke to me in the highest terms of Mr. Westinghouse and his character and accomplishments. On one occasion I was returning from work on the train from East Pittsburgh and Mr. Westinghouse was seated beside me. During the entire ride he spoke of my brother and of the confidence he had in him and his work. He said that when B. G. stated that

[1] See Appendix, p. 201ff.

a thing was all right, he knew that such was his honest opinion and since he knew that B. G. had the knowledge and ability thoroughly to analyze the problem, it was a pretty safe conclusion that it *was* all right. I mention this for the reason that both men were reticent regarding expressions of this kind and now that both have gone I like to remember that each placed so high a value on the services of the other."

THE MEDALS

When men are pleased to honor one of their fellows the medal is accepted as a fitting form for expressing appreciation and esteem. Twice Mr. Lamme received this evidence of recognition of merit. In the first instance it was the Thomas A. Edison gold medal awarded annually by the American Institute of Electrical Engineers. This he received on May 16, 1919, in the Auditorium of the Engineering Societies Building, New York City. The basis of the award was "For Invention and Development of Electrical Machinery."

In the second instance it was the Joseph Sullivant gold medal, awarded by Ohio State University at five year intervals, to the alumnus who should, in the judgment of the committee, have made, since the last award, the most notable contribution to the Liberal, the Fine or the Mechanic arts. Mr. Lamme received this medal on January the 11th, 1923, and since he received the initial award it was the logical conclusion that in the estimation of the various committees, he stood as the most distinguished alumnus of the Ohio State University at that date.

On the occasion of the presentation of the Edison medal Mr. B. A. Behrend made the following address:

"The development of the electrical industry in America is contemporaneous with the great industrial organization created by Mr. George Westinghouse and in the examination of its pioneers we are invariably arrested by the personality of Mr. Benjamin G. Lamme, whom it is our pleasure to honor tonight as the recipient of the Edison Medal. It is interesting to note that the medalist has just completed thirty years of service with the Westinghouse Company. There is something intensely significant in this fact. During these thirty years there have risen and there have vanished, like the colors in a kaleidoscope, many able and brilliant men who were connected with the Westinghouse Company in one capacity or another. Steadfast, patient and strong, there has remained the personality of Mr. Lamme to grow stronger with the years, so as finally to embody in himself, as it were, the thoughts of the engineering staff. I must confess that I have always enjoyed Mr. Lamme's versatile personality and that, in this sense, I may perhaps be accused of hero worship. Let the subject matter be one of engineering, one of politics, of the engaging problems of the day or of his favorite subject of archeology, his wholesome, sound sense and penetrative, subtle intellect will always impress his listeners. In point of illustration, here are a few pithy sentences taken at random from some of Mr. Lamme's writings. 'A brilliant mind with little persistency back of it will usually accomplish less than a much less brilliant mind backed by great persistency.'—'It is on account of specialization that it is so important that the young engineer of today obtain a broad knowledge

No. 1.—The Joseph Sullivant Medal—Ohio State University.

No. 2.—The Thomas A. Edison Medal—American Institute of Electrical Engineers.

of the fundamentals of his chosen line of engineering.'—'Such a course of advanced training would attract a great many students regardless of the fact that the training would be of little or no use to them.'

"Endowed with a most unusual memory, and a facility for mental calculation to a point of efficiency which makes him spurn the ubiquitous slide rule, he combines an imaginative faculty of a most curious order. Passionately fond of every variety of intricate puzzles, some of which he invented himself, devoted to reading imaginative stories and tales of which he has collected a large number in his library, he is forever dissecting and analyzing the problems which come within his scope.

"After this introduction to the medalist's personality, let me take you in mind to the fertile farm-land of Southern Ohio. There, by the Valley Pike in Clarke County, between Dayton and Springfield, about two score and ten years ago, Benjamin Garver Lamme was born. The life on that farm gave him the rugged constitution without which even his great talents would not have carried him through. 'The personal fly-wheel,' as he calls it, has often served him in good stead. Though extremely fond of squirrel-hunting, it is to be assumed that our young friend, Benjamin, must have liked other things better than farming. The early morning hours in 1887 and 1888, we are told, were often spent in poring over the pages of Sylvanus P. Thompson's *Dynamo-Electric Machinery*. Ohio State University, the cradle of a group of talented men,—Sabine, Storer, Feicht, Skinner, Scott, Mershon—was also Mr. Lamme's Alma Mater. We see him interested in everything, the coach of the men of lesser gifts, and graduating eventually as mechanical

engineer. After working on the flow of air and gas in pipes for the professor of geology, he applied for a position to Mr. Westinghouse, who had then just organized the Philadelphia Company, for the development and exploitation of the wells of natural gas, and thus secured a 'job' in Pittsburgh. After a few months, his employer, Mr. T. A. Gillespie, recommended him to the Electric Company, where we find him in 1889 in the testing department under Mr. Albert Schmid. The conscientious apprentice was instructed to polish the brass on an 1100 volt alternator and he showed his thoroughness in omitting no brass parts, not even the current-carrying brushes. To some unknown, fortunate circumstances we evidently owe the fact of his survival. With oil can in hand, scrupulously watching the bearings of the machines entrusted to his charge, Benjamin Lamme is a familiar sight in the memory of his early associates.

"Soon he became foreman of the testing department, and within a year his mind began to turn to inventions. From 1889, in increasing numbers from year to year, he obtained patents, reaching the rate of ten a year in 1898. Then we note a slight decline, only to attain a rate of sixteen patent applications a year in 1904. He has obtained at the present time the surprising total of 153.

"The earlier activity was associated with the development of the rotary converter, the later with that of the single-phase system. It is interesting here to compare the medalist's own estimate of his connection with these developments, and I quote:—

"'A year or so ago, in discussing the subject concerning which you have just written me, you mentioned that

the single-phase railway motor or system is the one by which I was best known. This appears to be true, but it has seemed to me unfortunate that it should be so, as I do not consider it as the one thing for which I should have the most credit.

"'As you may know, the rotary converter, as it stands today, covering almost the entire field of railway business, has reached its commanding position very greatly through my efforts. In the early days of the rotary converter, beginning in 1893, this device was looked upon with a great deal of doubt. In the early days of commercial application, I had to fight the battle for the converter pretty much by myself. That I made a successful fight is indicated by the fact that, in the period between 1898 and 1902, the Westinghouse Company was probably furnishing 75 percent of all the rotary converters then built and this business was becoming of very considerable proportions.'

"That he was early in this field is indicated by the fact that nearly all the patents taken out on rotary converters in those days were in his name. Many of the modern necessities in rotary converters were originated and first used by him in such apparatus. I may cite the damper as one of these examples. While it developed afterwards that Leblanc had a broad patent on the damper, yet at the time that Mr. Lamme developed and used it on the rotary converter, he was not aware of Leblanc's patent, and he may claim the credit of being the first to use such dampers to overcome fundamental rotary converter difficulties. The entire problem of hunting, which at one time threatened to put the rotary converter in the discard, was overcome by his early work. This, however, was only an incident in the development. When

it came to 60-cycle converters, he stood practically alone for many years, and his work has been instrumental in giving these machines the final high position which they now hold in the industry.

"He has always led in the battle for higher speeds, resulting in smaller, cheaper and more efficient converters. Therefore, considering all these things, I believe we may state that there is more credit due him than to any other one person for the present leading position which the rotary converter holds in the electrical field. Yet to consider this as a larger and more important work than the single-phase system, is, in my opinion, one of those strange vagaries of judgment often affecting us in connection with our own work.

"Again, he can claim to have been the leader in the direct-current railway motor development, as far as fixing types of apparatus is concerned. His earliest single-reduction railway motor, the Westinghouse No. 3, or rather its experimental predecessor, contained most of the fundamental features found in the present universal type of railway motor. This early motor was of approximately cylindrical type with four internal radial poles and was of the ironclad, or partially-enclosed type. This was a most radical departure from former constructions and at once overcame many of the earlier difficulties. The armature of the experimental machine, and of all later machines, was of the slotted type, with open slots and with machine-wound coils. At this time this was the only railway motor of the kind, and these features have since been universally adopted. In addition, the two-circuit or series type of armature winding was first developed and used on this motor and this type of wind-

ing is now in universal use for railway motors and almost all other multipolar machines of small and moderate size. Many other features of this early construction are still retained in modern railway motors. Therefore, he may claim credit for having established the present universal type of railway motor.

"Considering the direct-current generator, and particularly the railway generator, while there were more workers in this field, yet I feel that he should have much credit for establishing certain fundamental features in the early machines, which are retained or considered necessities even at the present time. He was the first to bring out the railway generator with slotted armature, both for the partially closed and the open slots. The form-wound armature coil of copper strap on edge, used with open slot direct-current armatures, was worked out by him. These earliest machines had a very high saturation in the armature teeth, in order to give stability and prevent distortion. This is also modern practice. When the limits of the two-circuit winding were reached and multiple windings began to be used extensively he devised in America the polyphase equalizing connections which are now considered a necessity in such machines. He also used the fractional pitch or chorded armature winding for direct-current machines, for the purpose of reducing the commutating constants, thus improving the range of operation without shifting the brushes. Therefore, in this line of apparatus, many of his original features of construction are still in use after twenty or twenty-five years of development.

"In his work on rotary converters, railway motors, and direct-current generators, it can surely be said that he

has had much to do with solving the problem of commutation, in the sense of determining the conditions which affect commutation and taking advantage of those characteristics and features which improve commutating conditions. In his papers on the theory of commutation, which were published by the Institute some years ago, I believe that he went considerably further into the subject than any one else had done up to that time. Quite a number of manufacturing engineers have taken up the method given in this paper and adopted it as a basis for their work, with very considerable benefits in the way of improved accuracy in their calculations, and increased outputs from a given amount of material. This paper, however, was more or less a result of all his previous experience and investigation on the subject, and it is simply an indication of how far he had gone into the general subject of commutation.

"While he was not among the first to develop induction motors, yet he was among the first to produce commercial induction motors, as his work began in the latter part of 1892. His leading work in this line was the recognition of the principles upon which the Westinghouse type 'C' motor was brought out. Up to that time (about 1895) it was generally held that the cage-wound motor was necessarily one with a small starting torque and that, in order to give high starting torque, it had to be made with a high secondary resistance and, therefore, high slip at normal load conditions. From his analysis of the design characteristics of the motor, he drew the conclusion that it was not a question of high secondary resistance and high slip, but one of the ratio of the reactance of the motor to the secondary resistance; that

is, by reducing the reactance instead of solely increasing the secondary resistance, high starting torque could be obtained, but at the expense of high starting input. He then proposed to design such motors with relatively much lower reactance than had hitherto been the case, and thus obtained starting torques of several times the normal running torque, and which he reduced by the use of lower starting voltages by autotransformers or autostarters. He worked up many experimental motors and proved this principle and also, through his method of analysis, determined how to reduce the reactance of the motor by using widely distributed windings, and by giving careful attention to the elements of the magnetic circuit, without materially increasing its size and cost. The results, put in practice, were the line of type 'C' motors. This radical type took the market and forced the situation to such an extent that it gave great impetus to the industrial application of polyphase currents.

"In the task of entering intimately into Mr. Lamme's work, we are aided fortunately by the possession of historical sketches, from his own pen, of the direct-current railway motor, the alternating-current generator, and the direct-current generator. To these we must refer the readers who wish to follow his work in the evolution of electrical machinery.[1] The universal type of single-reduction railway motor owes to him a great debt, and

[1] Published in *The Electric Journal* as follows:—"The Alternating-Current Generator in America," Vol. XI, February, 1914, p. 73; Mar., 1914, p. 120; April, 1914, p. 221. "The Development of the Direct-Current Generator in America," Vol. XII, February, 1915, p. 65; March, 1915, p. 115; April, 1915, p. 164; May, 1915, p. 212. "The Development of the Street Railway Motor in America," Vol. XV, October, 1918, p. 408; November, 1918, p. 454.

the rotary converter, especially the 60-cycle type is largely his creation. These conceptions found their early and practical expression in the 1500 Kw. rotary converters and the 5000 Kw. flywheel generators of the Interborough Rapid Transit Company's Manhattan Station. The electrical dimensions of the first vertical generators of the Cataract Construction Company were Mr. Lamme's part in the great power development at Niagara Falls, and it must be gratifying to him that he was thus connected with this interesting historical landmark in electric power transmission.

"There are many other fields in which he has had a hand. In the single-phase railway system, which he created, he deserves much credit for the fact that he successfully commutated alternating currents, after it had been quite definitely accepted as being one of the impracticable or impossible things. Of course, the development of the single-phase railway system was a direct result of the successful development of a commutating alternating-current motor. I think Mr. Lamme's leadership in the single-phase railway field has followed logically from his work in the transformation of Nikola Tesla's great creative ideas into commercial form. Mr. Lamme's principal, Mr. George Westinghouse, staunchly believed in the universal application of alternating currents and, as the difficulty of collecting alternating currents from two trolleys precluded, in the minds of the railroad men, the use of the polyphase system for railway work, Mr. Westinghouse and Mr. Lamme were led logically to the hope of developing a single-phase motor and adapting it to the railway field. This Mr. Lamme succeeded in doing originally by boldly adopting a low frequency of sixteen

cycles and designing for it a series-wound alternating-current commutator motor. A little later, however, he discarded this frequency and developed a 25-cycle single-phase motor, in which the use of resistance leads, reducing the transformer current in the short-circuited coils, is one of the salient elements. Thus resulted the 25-cycle single-phase railway motor so widely adopted in Europe and to a degree in America. Surely, the electrification of the New York, New Haven & Hartford Railway will always be associated with Mr. Lamme's name. No more bitter debates were ever heard on the floor of this Institute than those on the relative merits of the single-phase and the direct-current systems. Yet even the most relentless opponents of the single-phase system must admit in justice that Mr. Lamme's conception of railroad electrification stood for the use of a high voltage working conductor, as he recognized that the 600-volt third-rail system imposed too great a burden upon extensive railroad electrification. In setting himself the task of producing a motor which, while possessing the characteristics of the series direct-current motor, would operate on any trolley potential, however high, and in successfully solving this problem, and fearlessly applying it to the New Haven system with 11,000 volts on the working conductor, Mr. Lamme gave an impetus to the entire field of railroad electrification which will always be a milestone in the electrical industry, and in the field of transportation. Suffice it to say that the feverish activity in the development of rival systems, the general adoption of the overhead trolley, the doubling and quadrupling of the direct-current voltage thereon, have been due to Mr. Lamme's bold creative work. From it has sprung a new life, a

true 'instauratio magna,' in the electric transportation system the world over.

"Fifteen years have passed; both systems are still in use almost literally side by side, neither having succeeded in displacing the other, and prophecy would be idle. Be the fate of the single-phase system what it may, the medalist will always be remembered as its successful pioneer.

"With the advent of the steam turbine, Mr. Lamme turned his universal genius to the development of a type of turbo-alternator known as the "parallel-slot" type. It is one of the most ingenious designs which have been developed in this difficult art. While the types which he designed have not been enduring, yet they served as pace makers in the race towards the present high speeds and high economies resulting from such speeds. Therefore, I feel that he accomplished very considerable results in this field. Bold and venturesome, he urged increased speeds ever higher and higher, and tenaciously adhered to this policy. Great credit is due him for this fearless advocacy.

"Two years ago, when the Secretary of the United States Navy established the Naval Consulting Board, the Directors of the American Institute of Electrical Engineers were requested to nominate, and to recommend to the Secretary of the Navy, two members of their great body who, in their opinion, would best fulfil the requirements to be made of that newly created Board. It was a great pleasure for the present speaker that he then had an opportunity to commend Mr. Lamme's name. It is yet too early to appraise the good work done by Mr. Lamme on the Naval Consulting Board in his capacity as Chairman of the Inventions Committee.

"As an 'engineer-teacher,' he has had remarkable success. Storer, Renshaw, Hanker, Wilson, Hague, Laffoon, and others sat across the desk from him and profited by his sound sense and versatile intellect. The methods of calculation of electrical machinery in use at the Westinghouse works were inaugurated by him and still bear the marks of his individuality.

"Men like Benjamin Lamme are, as it were, the integrated personalities of an organization. They figuratively represent the aggregate. Such men are rare, indeed. When they appear among us, a dynamic equilibrium, so to speak, of intellect, shrewd common sense, and exceptional health, they present the picture of success. Somehow, in the twenty-two years I have known Benjamin Lamme, he has always called to mind the figure of another great American, of bygone years, who also worked in the field of electricity. In appearance even, but surely in his astute, sententious wisdom, I am reminded of Benjamin Franklin, more than a mere namesake.

"Mr. Lamme, it is a great pleasure to be here tonight and to pay you my homage in these words."

The Sullivant Medal[1]

The ceremonies attendant upon the award of the Joseph Sullivant medal at Ohio State University are thus described by Dr. William E. Henderson, Dean of the College of Arts, Philosophy and Science.

"Is there a college man anywhere whose eye fails to light and whose heart does not beat warm at the memory

[1] This is a University Medal, established by Dr. T. C. Mendenhall, formerly Professor of Physics and during the last years of his life a Trustee of the institution.

of some one preeminent occasion in college days when the event, the hour, the setting, and the expectation conspired to make a flawless memory? Is there an alumnus whose frequent returns to his alma mater, each with a little sense of disappointment, a consciousness that time had moved on, that change is everywhere evident, that friendships are less strong and true and more amateur than in his day, that the faculty has become impersonal and irresponsive to the step of the old graduate,—who cannot yet recall with deep satisfaction some one perfect return when warmth, and response, and genial spirits prevailed?

"Such a day was the 11th of January when a large group of the sons and daughters of thirty years ago, gathered back to witness the first award of the Joseph Sullivant medal to Benjamin Garver Lamme, of the class of '88, now a leading figure in the organization of the Westinghouse Electrical Company. Before that hour of three o'clock the Chapel was filled with old graduates, members of faculty, citizens of Columbus, and students; most of those who came just on the hour failed to gain access. It was an expectant and responsive audience. In the box to the right sat two daughters and a son of Joseph Sullivant, with family friends, while on the left was the engineer sister of Mr. Lamme (Bertha Lamme Feicht, '93), together with some of his intimate friends.

"The platform was tastefully decorated for the occasion, and President Thompson presided. It was an inspiring thought that he who has watched so many thousands of eager students go forth from college halls to the world of service, should now welcome back this one preeminent son to receive new recognition from his alma mater. In

words of dignity and in happy mood the President explained that the Sullivant medal would be bestowed at intervals of five years to that ex-student or graduate of the University or that member of faculty, who, since the last award, shall be judged to have completed a really notable piece of work in either the Liberal, the Fine, or the Mechanic Arts, the pure or applied Sciences including the various branches of Engineering. He described the mode of selecting the medalist,—the long deliberations of committees of the faculties; their weighing of many names and their recommendations to a committee of the graduate school; the selection by this body of three names from those nominated; and the reference of these names to a committee consisting of Dr. Elihu Thomson of the General Electric Company, Professor William Martin Wheeler of Harvard University and Director Fairbanks of the Boston Museum of Fine Arts. He exhibited the beautifully designed gold medal, a medallion portrait of Joseph Sullivant on the obverse, and on the reverse, the Seal of the University and the name of the medalist. Amid the generous applause of a rising audience he bestowed the medal upon Mr. Lamme.

Donor Makes Address

"The audience again spontaneously arose as Dr. T. C. Mendenhall, the donor of the medal, was presented. In words of rare sincerity and literary charm Dr. Mendenhall sketched the history of the pioneer Sullivant family from their early coming to Columbus to the final service on the Board of Trustees by Joseph Sullivant. He painted a vivid picture of the character of that family,

endowed with splendid native ability and possessed of the noble determination that lofty citizenship, public service, and devotion to the progress of science should be the ideals of the men who were laying the foundations of the frontier communities. With fidelity and loving devotion he traced the life of this younger son, less well-known to the world of science than his older brother, the engineer and botanist, who, nevertheless left a deeper impress upon Columbus through his many-sided scholarship, his philanthropy, his civic activity, his long service upon the school board, and his wise and farsighted leadership upon that first Board of Trustees of the University which assembled the original faculty and established the early policies. The children of this noble man, themselves honored residents of Columbus, must have felt a deep pride in this portrait. Certainly few who heard it could fail to discern the handing on of the torch of enthusiasm for all that is sound and true in scholarship from that Joseph Sullivant to our own Dr. Mendenhall; nor did we wonder that the qualities of heart and mind he so much revered and the life of service he so much respected which had made so deep an impression upon him as a young professor of Physics in those early days, should lead him in the full maturity of years to perpetuate the memory of Sullivant by the endowment of a memorial in recognition of illustrious attainment.

Dr. Elihu Thomson Speaks

"To Dr. Elihu Thomson, dean of electrical science in America, and a member of the committee that made the final choice of the medalist, fell the pleasant task of trac-

ing the developments in electricity during the past fifty years—a wonderful story as he told it, and one which made us marvel at the accomplishments within the span of our own lives. In this development Mr. Lamme has had a notable part as a leading figure in the Westinghouse Company, especially in the development of the railway motor and the synchronous converter, and Dr. Thomson paid a warm tribute to his attainments and to the high regard in which he is held by the leaders in electrical design throughout the country. A detailed account of Mr. Lamme's contributions to electrical science has recently appeared in a University publication (*The Ohio State Engineer*).

"Mr. Lamme's response was that of a modest man of action, not unaccustomed to recognition, but sincere, direct, and most appreciative of the honor conferred upon him by his alma mater. His thoughts reverted to those older and simpler days when he was a student. Dr. Scott sat upon the platform as he spoke, and by a happy coincidence the two men who Mr. Lamme said had been his greatest inspiration, though neither had been his teacher, were participators in the day's program—Dr. Mendenhall, the donor of the medal, and Dr. Thomson, who had just described his contributions to science. He paid a warm tribute to those who had guided his early college education, especially to Professors Robinson and N. W. Lord.

Dinner to Mr. Lamme

"In the evening a company of about 150—so limited because of the space available—gathered for dinner at

the Columbus Club. It was a company of old friends and associates together with some of the more recent faculty, all anxious to indicate their sense of the value of the occasion and to share in the more personal quality of informal speech. Messages from many sources were read by President Thompson congratulating Mr. Lamme as well as the University upon the occasion. Among these none were more appreciative, more expressive of regard for the man, and of the service of the University through the years, than were those from the officers, the colleagues, and the employees of the Westinghouse Company.

"Those who spoke after dinner were N. W. Storer, '91, Professor Harold Pender of the Department of Electrical Engineering of the University of Pennsylvania, Mr. Ambrose Swasey of Cleveland, Wm. W. Keifer, '86, Dr. Elihu Thomson, Mr. Lamme and Dr. Mendenhall."

In connection with the award of the Sullivant Medal, Professor Charles F. Scott wrote a Tribute to Mr. Lamme and his work which appeared in the Ohio State University Alumni Monthly. Since he is himself a pioneer in the field of electrical engineering and since he was the associate and friend of Mr. Lamme for thirty-five years in the University and in professional work, he is peculiarly qualified to evaluate Mr. Lamme's life work. His tribute is reproduced here:

Electrical Engineer—Pioneer, Leader, Educator.

"To Alumni of Ohio State University, the career of Benjamin Garver Lamme (1864–1924) is of peculiar significance, for among fifteen thousand and more of us

he was unanimously designated by the Board of Award to receive the first 'Joseph Sullivant Medal' in recognition of the value of his work to the world.

"He was Chief Engineer of the Westinghouse Electric and Manufacturing Company. He had received the Edison Medal, awarded annually by the American Institute of Electrical Engineers for electrical achievement. He had been one of its two representatives on the Naval Consulting Board during the war, serving as Chairman of the Inventions Committee which directed investigations and passed upon hundreds of schemes for the detection and destruction of submarines. These testify to his standing in his own profession as an engineer and inventor. But the Joseph Sullivant Medal brings in every field of activity in which graduates of more than forty classes are engaged; and the award went not to science or to art but to engineers, and to Lamme.

"It would be interesting to consult the Class Day prophecy of 1888 to note the career predicted for a member who held first rank in his classes but was little known outside the engineering group, who figured in no major or minor sports, who belonged to no fraternity or literary society, who took no part in social affairs, who was quiet and retiring, of few words and with nothing in appearance or manner indicating intrinsic capability. But the man ranking low in many qualities often counted essential to success, won in life achievement. Why?

"Many may imagine that an electrical engineer and inventor of pre-eminence must be a wizard of some sort; but he had no occult powers, he was no visionary. He was simple and unassuming in manner and mind and method. His magic wand for transforming new ideas

into realities was a trained mind. He could think clearly, analyse a problem into its fundamental elements, proceed directly from cause to effect or vice versa. He had vision, persistence, purpose. He was a productive thinker because he worked many hours and with high efficiency.

"My own recollection of student days gives a clew to his success. In the fall of '85 (I had received my A.B. in June) I took Analytic Mechanics with the engineering juniors. I can name only one student and recall but one outstanding incident. Professor Robinson said a page or so of the four or five pages of problems at end of the chapter would be a reasonable task. The next morning Lamme asked me how many I had done: I gave an apologetic answer and reversed the question. He quietly replied, 'Well, I didn't have anything else to do so I worked them all.'

"Nearly four years later, shortly after Lamme had begun work with the Westinghouse company on May 1, 1889, I remembered that sentence when Mr. Schmid asked me, 'Do you know that man Lamme? Can he figure?' And nearly forty years later, considerably more than half of which we worked in the same building—many years at adjacent desks, that sentence seems to reveal certain life characteristics—an interest in mathematical-mechanical problems, diligent effort at a self-imposed task far exceeding the normal expectation, a super-preparedness, a joy in accomplishment. His work as a student and as an engineer was not something apart from him, but it was a part of him.

"As Chief Engineer of his Company Lamme interviewed many young men. To determine present apti-

tudes he ascertained what they liked to do as boys, believing no one would make a real engineer who had not shown certain early traits and likings.

Notes Tell of Early Life

"He left some notes in which he traces his own career in a sort of self-analysis. His earliest recollections concern the hunting of curious stones in a plowed field. He continued to gather Indian relics, such as stone axes and hammers and arrow heads. In his will he leaves to the University his treasured and rich collection. He enjoyed building blocks and mechanical toys. He liked to work with tools and made little water wheels which he placed in the stream. He tinkered around machinery whenever opportunity appeared. The real pleasure was in finding out how things worked and why they were made in certain ways. These interests in childhood matured into interest in design in later years. An early aptitude later proving of highest importance was a liking for elementary mathematics. The wise teacher in an old-fashioned country school let the scholars work out things in their own way. Lamme enjoyed mental arithmetic, particularly the relations of numbers. (In later life he patented some magic square puzzles.) The multiplication table which we learned to 12, he learned to 25, then to 36. He developed curious and quick methods and a sense of proportion. While classmates in college thought mathematics 'came easy' to him, he was studying more hours than they did. He endeavored to get a physical conception of mathematical ideas and relationships. Later in life he had an uncanny way of working out a result in

his head before another man could get it on a slide rule. He used a slide rule for a short time but discarded it as the mechanical operation caused him to lose his quantitative sense and his facility for mental computation; furthermore, results were not retained mentally as they had been. Visualization and physical conception of principles and problems he employed constantly in his engineering work. Mathematical computation and analytical ability were closely associated. As a boy he directed his analytical ability to the characteristics of his schoolmates as well as to mathematical problems. A critical sense and persistency were other traits of youth which continued through life.

"He determined, when a boy, to be an engineer; he set about to make an engineer of himself and he did.

"Following his junior year, owing to the illness and death of his father, he spent a year at home on the farm near Springfield. After graduation from the Mechanical Engineering course he spent a half year at home, reading in spare hours Sylvanus Thompson's *Dynamo Electric Machinery*, a compendium of historical and descriptive information. He sought the underlying principles and got a grasp on the idea of the magnetic circuit, then little understood. When he came to the Electric Company in Pittsburgh, he had a preparedness which was unsuspected. His evenings for many years he spent largely in study. He has said that often his quick and confident answer to some new question has been attributed to an intuitive insight which others do not possess instead of the hours of painstaking analytical study which had already given him the answer.

"Opportunity is futile unless one is prepared to see it

and use it; Lamme was prepared. He graduated at the opportune moment when the electric current was establishing its usefulness for light and power and when the greatest problem was how to make bigger and better electrical machines; he was fortunate in the progressive leadership of George Westinghouse, who gave not only opportunity to design and construct, but incentive and impetus and inspiration; he was peculiarly fortunate in the early guidance and coöperation of Albert Schmid, a master in mechanical design.

Proved Equal to Opportunity

"Mr. Westinghouse had for two or three years been exploiting a new system, the alternating-current system. There were engineering criticism and commercial opposition from without and technical difficulties from within. The largest generators were less than two hundred horsepower; there were no commercial motors; there was no way to convert from alternating to direct current. The need and the opportunity were great. The design of new types of generators, of polyphase motors of various types, of synchronous converters, of an alternating-current railway system,—all these presented gigantic problems which Lamme met and solved. Present and proposed super-power systems and the utilization of power in industries and in railways employ the system on which he worked and most of the apparatus follows the designs which he inaugurated. He had many good assistants and much was done elsewhere, but Lamme was pioneer and leader, he originated new types and they have persisted. To him is due a far larger proportion of the

prominent types of apparatus now in use than to any other designer. And his judgment was so good that he spent little effort on types which soon disappeared.

"He never gave up. To him a defeat was a challenge. If trouble occurred, he at once sought a remedy—often he had already determined what to do if difficulties should arise. On one occasion there were serious troubles in an important installation. An official of the Company said, 'Lamme, this will be your monument or your tombstone.' This did not perturb him. Now it is his monument.

His Genius was Contagious

"Lamme was far more than an individual designer of particular machines, although he might well rest his reputation on the design of the single reduction railway motor, the machines exhibited at the World's Fair in Chicago, the generators which inaugurated hydro-electric power at Niagara, those for supplying power to the Elevated and Subway Systems in New York and the equipment which operates the New York, New Haven & Hartford Railroad. He says that his work was mostly analysis rather than design. He developed a system and method of simple rational design. He directed and taught others and during recent years his principal interest and activity and enthusiasm were directed to selecting and training men. He conducted a design class each year; he took active interest in *The Electric Journal* as chairman of its publication committee, suggesting topics and writers, critically reviewing manuscripts, and often summoning writers for conference, being particularly concerned in developing young engineers in clear

thinking and expression. He wrote and spoke on engineering education. Years ago the electrical engineer was an isolated individual. Now invention and development and design are conducted by groups of men who work together. Lamme started as an individual but he developed an organization and left in his department a hundred men with the advantage of his knowledge, his experience, his methods and his inspiration.

"Modest and retiring, he was a genial and inspiring companion to those who knew him. In later years, his reticence was somewhat overcome and he wrote papers and made some addresses. His clear and simple presentation, free from mathematical formulae made his writings highly appreciated. He regarded mathematics as a tool and did not exhibit his tools when the work was done.

"He never married, but made a home with his sisters. One of them, Miss Bertha, graduated from Ohio State in engineering and became a competent designer under her brother's direction, but ceased engineering to become Mrs. Feicht. He was a broad reader, he liked travel, he was fond of the best music and was interested in archaeology, he became an ardent and expert photographer and when he finally got an automobile he drove it nearly ten thousand miles in six months and knew all the roads within a hundred miles.

"Habitual optimism and indomitable courage waged a long struggle against an incurable disease. With hopeful cheer he concealed his early fears and to the last his knowledge from his immediate family. Again he had prepared for an emergency by deciding on plans for continuing his work and the provisions in his will. He died July 8th, 1924.

He Established Scholarships and Medals.

"Even associates and friends little suspected how sincere was his interest in education and his Alma Mater until revealed by his will. It provides for a medal to be awarded by the American Institute of Electrical Engineers for meritorious achievement in the development of electrical apparatus or machinery; also for a medal to be awarded by the Society for the Promotion of Engineering Education for accomplishment in technical teaching or actual advancement in the art of technical training. It provides a medal for meritorious achievement in engineering or the mechanical arts by a graduate of one of the technical departments of the Ohio State University; also for two scholarships to be awarded to the most capable students in the mechanical and electrical engineering courses during their senior year. All these awards—incentives for advancing the profession he loved—are to be made annually. There is also provision for continuing the education of eleven French orphans, wards of his for several years past. He visited them in France about a year ago, he heard from them often and his last replies—individual and personal—he dictated just three weeks before his death.

An Extraordinary Ordinary Man.

"Summing it all up, after an acquaintance of many years and after talking with many who have worked with him and know him best, he was an extraordinary ordinary man. He had developed ordinary qualities to an extraordinary degree.

"We all know the multiplication table, but he knew more of it and could use it better. He was an able mathematician but what he used most were mathematical reasoning and mental arithmetic. Most of us reason from effects back to causes, but he was wonderfully expert in getting directly back to the really fundamental starting point. He visualized and simplified his problems. He used short cut, direct methods in mathematics, in analysis, in action. He was unusually patient and persistent. When he had worked through to a conclusion, it became a conviction and he had the courage of his convictions.

"Lamme was one of those whose greatness lay in the perfection and practice of common traits and homely virtues. The qualities which made him great, while especially important to the engineer, would contribute to success in many fields."

OTHER TRIBUTES

Shoulder to shoulder for many years worked Mr. H. P. Davis and Mr. B. G. Lamme; the one directing that part of the Westinghouse Engineering work on appliances, switchboards, meters and control; and the other on generators, motors and converters. On the occasion of Mr. Lamme's death Mr. Davis paid him this tribute:

"A lifetime of fullest accomplishment was brought to a close when Benjamin G. Lamme died, and the electrical Industry has lost one of its greatest engineers. Brief as this statement is, it is full of meaning to those of us who have known him so well and so long, and have admired and loved him. My whole business life has been

one of close association with him, and his passing has made a void that never can be filled. On every side we shall see for many years to come reminders to keep his memory fresh, and as the years go on we shall understand better and better the quality and extent of his work.

"It was only to his associates and friends that he was really known. Modest almost to shyness, he attracted little attention from the general public. Indeed, although at the time of his death his genius was recognized throughout the world in engineering circles and he has been a leader in the design of electrical machinery for the past thirty years, until the last few years he was but little known even by the electrical fraternity.

"It would be relatively simple to write at length of the achievements of Mr. Lamme. They are many[1] and their history would be almost a history of the electrical industry and the development of the Westinghouse Electric & Mfg. Company of which he has been Chief Engineer for so many years. Not that he himself has been solely responsible for its engineering development—he would have been the first to deny it, although he has been personally responsible for many extremely important developments—but rather because the design methods which he originated and taught, the engineering principles which he established, and the men whom he helped to train, have had so large a part in the engineering work of that company.

"To write of the man himself is not, however, so simple. Of a retiring disposition, nevertheless the deep

[1] See "The Achievements of Benjamin G. Lamme," by B. A. Behrend in the *Journal* for June, 1919.

affection shown for him by all who had come into close contact with his inspiring personality, his ever readiness to help anyone who needed his assistance, and his training of young engineers—all are indications of his warm heart and deep interest in the welfare of his fellows. He was unfailingly courteous and considerate in his dealings with all.

"Mr. Lamme was endowed by nature with good health and an almost infallible memory, a keenly analytical mind, a great simplicity, a capacity for intense, continuous application and a high degree of engineering foresight. He possessed the ability to analyze a problem into its fundamental principles, and to see through a mass of details to the very heart of a problem. Hence, he was little apt to waste time or energy on stray bypaths. For instance, when asked to study an ingenious and complicated attempt to produce a multipole, direct-current generator without a commutator, and to explain why it would not work, he replied—'Isn't it sufficient to know that it will not work? I have no doubt I could point out in detail why this machine is inoperative, but it might take hours or even days. Fundamentally, all such attempts are in the same class with perpetual motion, or with attempts to transform from polyphase to single-phase without means of storing energy. I have a great deal of faith in the law of conservation of energy—why not let it go at that?'

"On another occasion a group of engineers had been working on a certain problem for several weeks. One of them outlined it to Mr. Lamme who almost at once suggested a solution. This engineer afterwards said, 'I would give three months' pay to have seen that before

Mr. Lamme did! It is so obvious when once pointed out, yet none of us saw it.'

"He possessed the courage of his convictions and would stand vigorously and persistently for a principle or plan of action which he believed sound. He was a pioneer in street railway electrification; his motor designs were epoch-making, and the principles he established in those early days are still standard practice.

"Almost alone he fought the battle for the rotary converter at a time when engine driven direct-current generators were in practically universal use for both railway and Edison three-wire systems. Most of the earlier converter patents are in his name. When the 25-cycle converter had become firmly established, he again took up the cudgel for the 60-cycle converter and was as completely successful.

"He was a thorough believer in co-operation—always willing to co-operate with others and to give fullest credit to those who helped him. He was bold in imagination, in conception and in accomplishment, and untiring in the persistence with which he followed through a problem once undertaken. On one occasion a novel and untried type of winding was proposed for a machine of large size. After Mr. Lamme had given his approval, it was suggested that an experimental model of 50 to 100 hp. size be first constructed to see how it would operate. But Mr. Lamme had analyzed the winding and could see no reason for wasting time or money experimenting to test an idea, when his analytical judgment assured him that the basic principle was sound. The first winding of this type was built to a large scale and proved even more successful than was anticipated.

"Mr. Lamme's high mathematical ability was not widely recognized for two reasons. In the first place, his own method of solving problems was notably simple and direct. As he himself once said—'One great advantage of a thorough understanding of calculus is that you know enough to let it alone when you don't need it. Only a novice uses calculus to solve arithmetic problems.' Another reason is that his writings are the essence of simplicity. He endeavored to make every statement simple and easily understood, and whenever possible thought and wrote in terms of physical rather than mathematical concepts. Hence, while he was not a voluminous writer, whatever he wrote was widely and eagerly read. As an example may be considered his paper on the induction motor which has become an engineering classic on this subject; which has been reprinted many times; and which, although written more than a quarter of a century ago, is still unsurpassed as an analysis of the polyphase induction motor and is as up-to-date now as when it was written. This paper was based on a mass of mathematical equations, but Mr. Lamme believed that mathematics form the tools of his profession and that the tools should be put out of sight before the finished product is exhibited.

"As a developer of young men, Mr. Lamme's influence will long be felt. He constantly urged that the engineering colleges should teach mainly the fundamental principles of physics and mathematics, believing that the graduate who was well grounded in these subjects could advance more quickly in his chosen profession than one less thoroughly grounded though having a smattering of specialized information. In furtherance of this idea, Mr.

Lamme selected from the college graduates who came to the Westinghouse Company, those whom he considered most adaptable to advanced design and personally taught to them the design principles and methods he himself had made so successful. Perhaps no clearer exposition of his character can be made than is given by his own statement regarding the training of young men, which is quoted in connection with the frontispiece of this book. Mr. Lamme formed a living example of these principles which he taught to others.

"In his private life Mr. Lamme had a wide variety of interests. He was a broad reader, fond of travel. His tastes ranged from archaeology to the weirdest, most imaginative tales he could find, of which he had a big collection. He was intensely interested in every variety of intricate puzzles, some of which he invented. He was an ardent amateur photographer, having a fine collection of cameras, lenses, enlarging equipment, etc., and made a special study of telephotography. While he seldom took any of his work home with him, his chief recreation for many years consisted in working out and studying over problems and methods of design, spending from three to five hours a night in this way. He was not particularly interested in outdoor sports, yet when he took up automobiling he developed at once the intensity that was characteristic, and quickly became an authority on routes and road conditions throughout western Pennsylvania and eastern Ohio.

"The world has lost a great engineer. His associates have lost a true friend and never-failing source of inspiration."

For the opportunity to develop his genius and exercise it to the utmost Mr. Lamme recognized that he owed much to George Westinghouse and to the organization and personnel of the Westinghouse Company. No member of that organization was in a better position to appreciate Mr. Lamme's work and its value to the organization than its president, Mr. E. M. Herr, who took up the guidance of that organization when Mr. Westinghouse laid it down. Mr. Herr says of Mr. Lamme:

"Benjamin Garver Lamme was born on a farm near Springfield, Ohio, and received his early education in the country schools of that vicinity. He later entered Ohio State University, and graduated with the class of 1888 as a mechanical engineer. The following year he entered the Testing Department of the Westinghouse Electric & Manufacturing Company, and soon evinced a remarkable talent for designing, with which he was closely identified throughout the balance of his life.

"His conspicuous ability as a mathematician and his keen analytical mind, coupled with absolute dependability and yet boldness in endeavor, soon marked him for advancement. He rapidly progressed to the position of Assistant Chief Engineer, and in 1903 to the position of Chief Engineer of the Company, which position he held at the time of his death.

"Mr. Lamme was a leader in the development of many of the most important lines of the greatly complicated and widely extended field of electrical engineering. His original inventions—several of them epoch-making in their scope—are covered by more than one hundred and fifty patents. During the life of Mr. George Westing-

house, Mr. Lamme was constantly called upon by him for development and pioneer work in the electrical field and the confidence and trust thus placed in him were amply justified by the many remarkable inventions produced in both the alternating and direct current fields.

"In addition to his duties as Chief Engineer of the Westinghouse Electric & Mfg. Company, he was for many years chairman of a standing committee which passed upon the character and value of all inventions brought to the attention of the Company. Furthermore, when, at the beginning of the war, the American Institute of Electrical Engineers was requested by the Secretary of the Navy to name two men of the highest engineering attainments as members of the Naval Consulting Board, then being formed, it chose Mr. Lamme as one of these men from a membership of more than ten thousand engineers, thus conferring upon him a most distinguished honor and giving him the highest rank as an engineer and inventor.

"In 1919 the highest honor in the gift of the American Institute of Electrical Engineers—the Edison Medal—was conferred upon him.

"Mr. Lamme's career as an engineer and inventor is too well known to need detailed repetition here. He so long occupied a place of great prominence in his profession and was so signally honored at various times that his name is known and his engineering achievements are recognized among electrical engineers throughout the world as being of the highest order.

"With all his honors and attainments, Mr. Lamme was modest almost to shyness, yet never failed to take a great interest in young engineers. Each year he personally

examined a large number of the young graduates from the various technical schools and colleges, who were beginning their professional work with the company, and selected those who gave evidence of ability to develop rapidly to form a class in higher engineering studies, which he himself taught. His ability as a teacher in his chosen profession was remarkable. He had developed many short, original, and exceptionally direct methods of calculation, and always having among his students a selected few, he produced engineers of great analytical ability and resourcefulness in design. Many of the Company's most talented engineers can testify to the remarkable stimulus gained by their personal contact with Mr. Lamme through this class.

"As showing the purpose which actuated his life in training these young men, I quote from Mr. Lamme's own words—

"'I have seen many of these men grow from pupils to assistants and associates and one of my greatest pleasures has been to see this growth. I have allowed no petty jealousies to interfere with their development, and I have always felt that as they grew, so would I grow with them. I have aimed to instill in them fundamental ideas of engineering honesty and honor, square dealing and fair fighting, that there should be pride in accomplishment and that true engineering means much more than merely making a living or obtaining an income—that it means advancement of the art for the benefit of mankind.'

"His reading and knowledge were by no means confined to engineering subjects alone. His keen mind worked in many fields of intellectual endeavor and gave him a

breadth of learning and information rarely found. He was courageous, resourceful, and persistent in his work, courteous and considerate in his dealings with all, and was held in the highest esteem by those who knew him best."

THE NAVAL CONSULTING BOARD OF THE UNITED STATES

In July, 1915, the World War had been raging for nearly a year with increasing violence. Most Americans were by this time impressed with the menace to the United States. Something had to be done to put America into a position where she could defend herself. The press accounts gave daily news of the part that science and invention was playing in the war, of the introduction of new weapons, of the fact that those waging the war were utilizing the best inventive and scientific talent of their respective countries.

A progressive Secretary of the Navy, Josephus Daniels, realizing the great part that inventions and new devices were playing in the war, sought to find a method of helping to meet the situation. In a letter to Mr. Thomas A. Edison on July 7, 1915, among other things he stated that there was no particular place or particular body of men relieved of other work and charged solely with devising new things themselves or perfecting the crude ideas that were submitted to the Navy Department by the naturally inventive people of the United States and that he had in mind a general plan of organizing a department for the Navy which met with the ideas of Mr. Edison, previously expressed, for such a department for the government in general. After some correspondence, Mr. Edison con-

The Naval Consulting Board of the United States.

Mr. Lamme is on the extreme right end of the front row.

sented to head such a board. Secretary Daniels then wrote to the Presidents of the eleven largest engineering Societies in the United States and asked each, as president of the organization, to secure the selection of two of its members to serve on this Naval advisory board, further stating, "the judgment of your members as to who is most qualified among you to serve on this board will be far better than my own."

The names of the Societies selecting the members of what was afterward officially named the Naval Consulting Board of the United States and the members selected by them and by the Secretary of the Navy, were as follows:

Thomas A. Edison—Selected by the Secretary of the Navy.

Dr. M. R. Hutchison—Selected by the Secretary of the Navy.

American Chemical Society—Dr. L. H. Baekeland and Dr. W. R. Whitney.

American Mathematical Society—R. S. Woodward and Arthur G. Webster.

American Society of Civil Engineers—A. M. Hunt and Alfred Craven.

American Aeronautical Society—M. B. Sellers and Hudson Maxim.

Inventors Guild—Thomas Robins and Peter Cooper Hewitt.

American Society of Automotive Engineers—Howard E. Coffin and Andrew L. Riker.

American Institute of Mining Engineers—William L. Saunders and Benjamin B. Thayer.

American Electro Chemical Society—Lawrence Addicks and Prof. Jos. W. Richards.

American Society of Mechanical Engineers—W. L. R. Emmet and Spencer Miller.

American Society of Aeronautic Engineers—Elmer H. Sperry and Henry A. Wise Wood.

War Committee of Technical Societies—D. W. Brunton.

American Institute of Electrical Engineers—B. G. Lamme and Frank J. Sprague.

The signal honor of representing the American Institute of Electrical Engineers, a body of more than ten thousand engineers, was conferred on Mr. Lamme by the Board of Directors of the Society after first obtaining suggestions from a large group of representative members consisting of the past presidents, members of the board of directors, and the chairmen of the branch organizations located in the principal cities of the country.

After its organization, the work of the board was distributed among twenty-one committees. Of these committees Mr. Lamme served on five, namely: (1) Electricity; (2) Production, Organization, Manufacture and Standardization; (3) Steam Engineering and Ship Propulsion; (4) Metallurgy; (5) Committee on Special Problems. He acted as chairman of the last committee which did outstanding work on the detection of submarines.

From October 7, 1915, until August 29, 1916, the Naval Consulting Board was an unofficial body but by act of Congress on the latter date it was legalized and became a regular branch of the service and so functioned until the close of the war. The work accomplished by the board is

Portrait in 1916.

described in a volume entitled *Naval Consulting Board of the United States* by Lloyd N. Scott, late captain, U. S. A., and issued by the Government Printing Office at Washington, D. C., in 1920. At the close of the book the author says, "The patriotic and wholehearted service which members of the Naval Consulting Board at the request of the Secretary of the Navy rendered to the government during one of the most trying periods in the history of the United States, was something to reinforce our faith in human nature and the democratic institutions on which our government is founded."

ARCHÆOLOGICAL COLLECTION

In the second paragraph of his story Mr. Lamme mentions the fact that one of his earliest recollections, when he was about four years old, was of going out into one of his father's fields to look for certain curious stones. Again in Chapter IX he speaks of the great interest which he had in archæology. There is no doubt that this interest was created and stimulated by the evidences and relics of the race of Mound Builders which exist in greater abundance in Ohio than in almost any other part of the United States. Particularly in southern and western Ohio near Mr. Lamme's birthplace, the number and variety of these monuments fill any thoughtful observer, such as he was, with speculations as to whence came and whither went the race of people who built these mounds. The vicinity of the mounds and in fact all the surrounding country abounded in stone relics of various races of aborigines. These constituted the "curious stones" which

Mr. Lamme as a boy collected in the manner that boys gather up all sorts of treasures of like nature. He continued this collection throughout his life and in his will bequeathed to Ohio State University a collection of several hundred spear points, arrowheads, axes and celts of great value as a permanent record of primitive man in Ohio for future archæologists.

The following list of the specimens in this collection has been furnished by the Curator of the Museum of the Ohio State Archæological and Historical Society where the collection is now deposited.

Item	*Description*
1.	44 Grooved stone axes.
2.	24 Celts (stone hatchets)
3.	5 Stone chisels, good specimens of this implement.
4.	28 Pestles.
5.	58 Grooved stone hammers.
6.	19 Hammer-stones.
7.	Unfinished Ceremonial of polished stone, flattened tube type.
8.	Unfinished Platform Pipe, pink oolitic fire clay.
9.	Pendant; banded slate, one hole type.
10.	Plummet.
11.	Unfinished Banner-stone and other slate pieces.
12.	Spearpoint of flint; 3½ inches long, broad type with long barbs.
13.	Arrowpoints, about 300 of flint.
14.	Spearpoints, 47 flint points.
15.	Arrowpoints, 24 of white quartz.
16.	18 "Bunts," chipped flint.
17.	Flint Drills, 6 unbroken, and 12 pieces.
18.	75 Blanks and roughed out blades.
19.	Lot of fragments of arrowpoints.

LIST OF IMAGINATIVE BOOKS

For many years Mr. Lamme and Charles P. Steinmetz were friends and shared common interests. One of these was a liking for imaginative fiction. Mr. Steinmetz once asked Mr. Lamme for a list of his books of this type. The list given is produced here with the original comments on the kind of story and its rating as it impressed him.

Author	*Title*	*Type of Story*	*Rating*
H. G. Wells	Time Machine	Weird, imaginative	Very good
	Island of Dr. Moreau	Weird, imaginative	Very good
	Tales of Space and Time	Weird, imaginative	Very good
	When the Sleeper Wakes	A Story of the Future	Very good
	War of the Worlds	Weird, imaginative	Very good
	Thirty Strange Stories	Weird, imaginative	Very good
	First Man in the Moon	Weird, imaginative	Very good
	Food of the Gods	Weird, imaginative	Very good
	Twelve Stories and a Dream	Weird, imaginative	Very good
	The Invisible Man	Weird, imaginative	Very good
	The War in the Air	Weird, imaginative	Very good
	Men Like Gods	Weird, imaginative	Very good
	The Country of the Blind	Short stories	Very good

Author	*Title*	*Type of Story*	*Rating*
Bram Stoker	Dracula	Vampire Story, weird, imaginative	Very good
	The Jewel of Seven Stars	Egyptian Story, weird, imaginative	Good
Lester Arnold	Phra, the Phoenician	Re-incarnation Story, weird	Very good
	Lepidus, the Centurion	Re-incarnation Story, weird	Fair
Thos. Janvier	The Aztec Treasure House	Adventure—imaginative	Good
	In the Sargasso Sea	Adventure—imaginative	Good
Mitchell	The Last American	Future	Fair
	The Villa Claudia	Imaginative	Fair
Conan Doyle	The White Company	Historical Adventure	Good
	Sir Nigel	Historical Adventure	Good
	The Last Galley	Short Stories—Old Time	Good
	Strange Secrets	Weird Short Stories	Good
	The Lost World	Prehistoric Life and Imaginative	Very Good
Stanley Waterloo	The Story of Ab	Prehistoric Story	Good
Elie Berthet	The Prehistoric World	Prehistoric	Fair
Gouverneur Morris	Pagan's Progress	Prehistoric	Fair
	Yellow Men and Gold	Adventure, imaginative	Good
Le Queux	Eye of Istar	Adventure, imaginative	Very good
	Zoraida	Adventure, imaginative	Good
	Closed Book	Adventure, imaginative	Fair
Anstey	The Brass Bottle	A Genii Story	Good
	Vice Versa	Imaginative	Fair
	The Tinted Venus	Imaginative	Fair
Cutlyffe Hyne	The Lost Continent	A Story of old Atlantis	Good
Clarke Russell	Frozen Pirate	Imaginative Sea Story	Very good
	The Flying Dutchman	Imaginative Sea Story	Fair

Author	*Title*	*Type of Story*	*Rating*
Theophile Gautier	One of Cleopatra's Nights	Short Stories, imaginative	Very good
Robert Chambers	The King in Yellow	Weird Tales	Good
	The Maker of Moons	Weird Tales	Good
	The Mystery of Choice	Weird Tales	Good
Lloyd Osborne	The Adventurer	Search for Lost City	Very good
Buchan	The Great Diamond Pipe	Adventure Story	Good
Flaubert	Salambo	Old Carthage	Very good
De Mille	Strange Manuscript Found in a Copper Cylinder	Weird Story	Good
Stockton	Vizier of the Two-Horned Alexander	Imaginative	Fair
	Great Stone of Sardis	Imaginative	Fair
W. W. Jacobs	Lady of the Barge	Short Stories	Good
Mark Twain	Captain Stormfield's Visit to Heaven	Odd and funny	Good
Lilienkranz	Ward of King Canute	Historical	Fair
	Randvar the Songsmith	Historical	Fair
Jack London	Before Adam	Prehistoric	Good
	South Sea Tales	Odd	Fair
Orczy	Gates of Kamt	Imaginative	Fair
Haggard	She	Imaginative	Good
	Ayésha	Imaginative	Good
	King Solomon's Mines	Imaginative	Good
	People of the Mist	Imaginative	Good
	Montezuma's Daughter	Imaginative	Good
	Allan Quatermain	Imaginative	Good
	Marie—Zulu Stories	Semi-historical and imaginative	Good
	Child of the Storm—Zulu Stories	Semi-historical and imaginative	Good

Author	*Title*	*Type of Story*	*Rating*
Haggard—*Continued*	Finished—Zulu Stories	Semi-historical and imaginative	Good
	When the World Shook	Imaginative, prehistoric	Fair
	The World's Desire	Old Egyptian	Good
	Smith and the Pharaohs	Short Stories	Good
R. L. Stevenson	Treasure Island	Adventure (Classic)	Very Good
Marryatt	Pacha of Many Tales	A Modern Arabian Night	Good
G. A. England	Darkness and Dawn	Very weird, imaginative	Very good
Frankau	Seeds of Enchantment	Weird Adventure	Good
Shanks	People of the Ruins	A Story of the Future	Very good
Merritt	The Moon Pool	Weird, imaginative	Very good
Jensen	The Long Journey	Prehistoric	Fair
Burroughs	The Princess of Mars	Wild and wooly	Fair
	Gods of Mars	Wild and wooly	Fair
	War Lord of Mars	Wild and wooly	Fair
	Thuvia Maid of Mars	Wild and wooly	Fair
	Chessmen of Mars	Wild and wooly	Fair
	Tarzan Stories—(Five or six in number)	Wild and wooly	Fair
	At the Earth's Core	Wild and wooly	Fair
Vivian	The Wonder City	Adventure, imaginative	Good
E. V. Odle	The Clockwork Man	Weird	Good
Ray Cummings	The Girl in the Golden Atom	Weird, scientific	Very good
Burton E. Stevenson	A King in Babylon	Weird Movie Story	Very good

THE "LAMINATIONS"[1]

One of Mr. Lamme's chief interests was the personnel of the design departments of the Westinghouse Company's engineering organization. In the interest of the engineering department he organized and personally instructed a school, in which classes of five to ten men devoted their whole time to the intensive study of the design of electrical machinery for about six months. The first class was organized with five men during 1912. It is characteristic of Mr. Lamme in this undertaking that he tested his accomplishment with human material, just as he did when he worked with iron and copper. He watched the progress of his first pupils for three years before forming the second class in 1915. Other classes followed at intervals until in all one hundred and eighteen men were given this special training.

Mr. Lamme's method of instruction was to outline orally a procedure for solving a certain problem. When the class had analyzed his method and had worked out its details, he discussed the problem until he was satisfied that it was well understood. He then furnished the data on a number of machines, and the class made calculations, using the method just derived, and the results were checked against tests. The fundamental of all of Mr. Lamme's design methods was the constant retention, from beginning to end, of the physical conception of the problem.

Each year Mr. Lamme interviewed from seventy-five to one hundred and fifty engineering graduates who had come to the Westinghouse engineering department. He

[1] By one of them.

chose the members of his school largely as a result of those interviews. However, each man who was selected, had also to solve eight or ten rather difficult problems, which involved mathematics as advanced as the usual college calculus. In the interviews Mr. Lamme usually asked about the boyhood activities of the man. He believed that the love for designing and building machines was revealed in a boy's interest in building simpler things. He differentiated between the love of building the toy and that of operating it when it was completed. His selection of men often hinged on the answers to these questions. Mr. Lamme depended on his methods to penetrate any exterior, because the men he chose differed very widely in appearance, and in their preferences, and ambitions.

It was natural that young engineers should be attracted to Mr. Lamme because of his position and his past accomplishments. But association with him always increased this attraction, largely because of his intense interest in the subjects he discussed. Whether the subject pertained to engineering or not, he carefully analyzed it and invariably had an original and plausible opinion. The enjoyment found in association with him led to the organization of the "Laminations," a club composed of the young men who had had training in electrical design under his personal supervision.

The organization of the Laminations took place after the first school in 1912. Several meetings of a technical nature were held, but all activity lapsed until 1917, when a reorganization was effected with the thirty-six men who had by that time been through the design schools. Mr. Lamme was sufficiently interested in this group to attend

the early meetings, and to suggest subjects for discussion. He rather discouraged a program consisting wholly of technical subjects. However, the importance of papers read before various societies had greatly increased in Mr. Lamme's estimation during the last ten years of his life, and he suggested that such papers be presented to the Laminations first to develop weaknessess not apparent to the author. He preferred curious problems, distinctly different from every day work, and proposed one meeting each year in the nature of a banquet, theater party, or river excursion.

The name, "Laminations," had been chosen by the six men who first organized. It was adopted by the larger group when meetings were resumed. Mr. Lamme criticised the selection as a bad pun on his name. The members took his comments more seriously than Mr. Lamme had meant them, as was later discovered, and a committee was appointed to propose an alternate. After several deliberations the committee reported that "Laminations" was the best name, and it was retained. Aside from the apparent connection with Mr. Lamme, the name was found to imply to electrical men, the desirable characteristic of a number of individual units, bound together to render service, just as many thin sheets of steel, termed laminations, are built up into a single mass or magnetic core.

The Laminations met for several years, and experimented with various types of programs. Technical papers were read, the heads of various departments in the Westinghouse Company were asked to discuss their work, electrical problems pertaining to the Army and Navy were investigated, and several banquets were held. But attendance soon showed that Mr. Lamme was by far

the most popular speaker who could be obtained. In fact he proved to be the tie which was necessary to the life of the group.

In addition to the individual subjects Mr. Lamme discussed, he gave three series of historical talks on the electrical industry. His first series dealt with progress as expressed in the design of the different electrical machines. He reviewed the transition of each important machine from its invention, through its experimental development, to the present day. In the second group he told of the electrical manufacturing companies which rose to prominence and continued successfully or disappeared. He often showed technical reasons for their success or failure, as well as financial reasons. The last series of talks that Mr. Lamme gave the Laminations concerned the prominent men of the electrical industry. It was astounding with how many of them he had had personal contact. He chose about fifty of the engineers who had contributed most to electrical development in America, and told of their accomplishments or idiosyncrasies, while he showed their photographs on the screen. Mr. Lamme's talks were always informal, and questions were in order at any time. His attitude made him thoroughly approachable, but deprived him of none of his dignity.

Mr. Lamme attended the banquets of the Laminations,[1] and obtained speakers, as he did for other meetings, who could not have been obtained without his influence. He contributed more than anyone else to the conversation during the dinner, and to the stories told later. When a canvas was made to learn which members would at-

[1] Practically the only exception ever made to his rule not to attend dinners or banquets.

tend a banquet, the first question each asked was, "Will Mr. Lamme be there?"

The history of the Laminations and their contact with Mr. Lamme cannot but reveal various side lights on his character and characteristics. Although rather reticent socially, he appealed to every one of these young men. Any assembly, at which he was present, drew all of the men who could attend; any other speaker on any subject was very apt to lack a full audience. The quality which one man most appreciated might be unnoticed by another admirer. One looked to him for friendly advice about his progress in the company's organization. Another came to receive most able aid on purely technical problems, and with this aid would be combined encouragement which made the darkest outlook seem possible of penetration. Another respected his prodigious memory, or his speed and accuracy in mental calculations. Another enjoyed his anecdotes, and constant good humor. Another was interested in his hobbies, and the thoroughness with which he pursued them. Another admired the unity of purpose which guided his life, while another found inspiration in his intense loyalty to his company. But each man found that some elevating thought remained, after contact with this perpetual student and teacher had been lost.

THE FATHERLESS CHILDREN OF FRANCE

To the literary executor of Mr. Lamme, who must examine the correspondence files which he left covering many subjects in many ways, there comes at no time such a crushing sense of human loss as in looking over what remains of his interest in eleven French orphans whom he took into his heart after the World War and to whose

daily necessities and education he contributed generously for several years. His was not the perfunctory gift of the professional philanthropist but he gave himself as well as his money. In the correspondence carefully filed with each child's name are many letters in childish script conveying in quaint conventional phrases their appreciation of what he was doing for them. In the back of each file is an envelope with little faded bunches of flowers, photographs of the child, perhaps decked for his first communion, and bright colored or embroidered postal cards such as the French custom suggests at Christmas, New Year, Easter and other seasons. Now and then there is a letter from a mother asking a blessing on the kind hearted benefactor so far away who is doing much to lighten the heavy burden of sorrow and toil left as the aftermath of the worst struggle in history.

A bachelor himself more from chance than from choice, Mr. Lamme never failed to keep his interest in children and all the children with whom he came in direct contact knew him and loved him. It is not surprising that when the appeal was made to him in 1918 by the Fraternité Franco-Américaine to enroll in a movement called "The Fatherless Children of France" he did so most generously and became responsible during the period of their activity for five boys and five girls at the stipulated rate. Later he added the little sister of one of the ten. After the society discontinued its work, he continued his regular contributions to his wards and at his death remembered each with a bequest. From his correspondence with them are here quoted one of his letters and one of theirs, which are typical of very many of each in the correspondence file for each child.

"MAY 22, 1922.

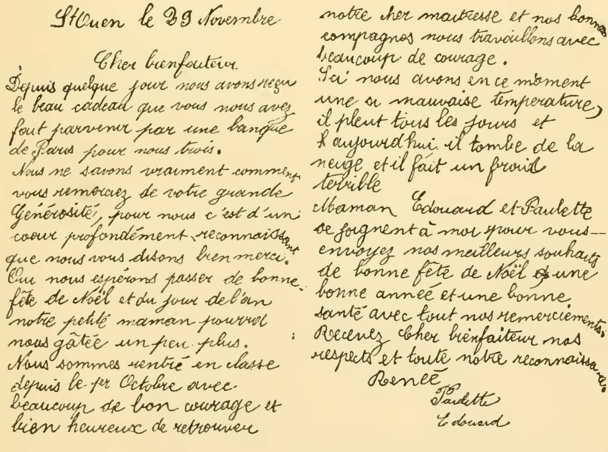
JEANNE—
DEAR LITTLE FRIEND:—

I was glad to receive your mother's letter of December 22nd and also your two very pretty cards.

I hope you study well as I am sure it will please your mother very much. All mothers take great pleasure in the success of their children and I know you are doing all you can to help your mother in every way. I am sorry to hear of your mother's ill health and hope she will improve very soon. For this reason also, you should do everything possible to make her days pleasant and happy.

You must now be quite a big girl, for your ninth birthday has just passed and I hope you had a very happy one.

With the best of wishes, I am,
Sincerely yours,
B. G. LAMME."



St Ouen le 29 Novembre

Cher bienfaiteur.
Depuis quelque jour nous avons reçu le beau cadeau que vous nous avez fait parvenir par une banque de Paris pour nous trois.
Nous ne savons vraiment comment vous remerciez de votre grande Générosité, pour nous c'est d'un coeur profondément reconnaissant que nous vous disons bien merci.
Oui nous espérons passer de bonne fête de Noël et du jour de l'an notre petite maman pourrait nous gâtée un peu plus.
Nous sommes rentré en classe depuis le 1er Octobre avec beaucoup de bon courage et bien heureux de retrouver notre cher maitresse et nos bonnes compagnes nous travaillons avec beaucoup de courage.
Ici nous avons en ce moment une si mauvaise temperature, il pleut tous les jours et aujourd'hui il tombe de la neige et il fait un froid terrible
Maman Edouard et Paulette se joignent à moi pour vous envoyez nos meilleurs souhaits de bonne fête de Noël et une bonne année et une bonne santé avec tout nos remerciements.
Recevez cher bienfaiteur nos respects et toute notre reconnaissance.
Renée
Paulette
Edouard

Letter to Mr. Lamme from French orphans who had been his wards

(TRANSLATION)

"ST. OUEN, NOVEMBER 29.

DEAR BENEFACTOR:

A few days ago we received the nice present that you forwarded to us through the Bank of Paris, for the three of us. We really do not know how to thank you for your great generosity to us. From the bottom of our hearts we wish to thank you.

Yes, we hope to spend a Merry Christmas and at New Years probably mother will spoil us a little more.

We returned to school on the first of October with enthusiasm and quite happy to meet again our dear teacher and our good friends. We work with great zeal.

At the present time we are having very bad weather here. It rains all the time and today it is snowing which makes it very cold.

Mother, Paulette and Edouard join me in sending you our best wishes for a Merry Christmas, a Happy New Year and good health.

With all our thanks, receive dear benefactor, our respect and appreciation.

RENEE, PAULETTE AND EDOUARD

PATENTS

During his lifetime Mr. Lamme was awarded one hundred and sixty-two patents. The list includes many that were of the greatest practical service in the development of the electrical art. His main interest lay in the problem itself and its solution, and he has himself remarked that the resulting patent was in the nature of a "by-product." Since many of his own inventions were the direct result of overcoming difficulties, he had formed a criterion for judging the novelty and practical worth of

his own and other's ideas which enabled him to decide quickly to what extent they merited patent protection. When a discussion arose as to whether certain work constituted invention or should be filed upon, he inquired whether any immediate changes in design were contemplated as a result of the new idea. If it was to be put into practice at once, he held it to be a common-sense argument that either an entirely new result was to be accomplished or an old result improved, and hence there was evidence of invention which should be protected. This standard might be considered too broad by those whose business it is to interpret the niceties of patent law, but to his associates it was another evidence of the broad common sense and essential sanity that marked all of his thinking.

A bare list of the titles of his patents does not give sufficient indication of their content and an attempt to abstract them individually would necessitate covering much of the ground he has himself covered, but it is interesting in running over such a list to realize how truly he was Chief Engineer of his organization in the sense that at some period or other he was as intimately concerned with the design details of the apparatus in the various departments as were the specialized designers on the different lines themselves. Two subjects were full of interest for him at all stages of his thirty-five years of work. These were electric railway work and induction motors. He might for a considerable period, even several years, lay them aside for something which claimed his immediate attention to a greater degree, but they were, in a sense, his earliest interest and he kept coming back to them. In the beginning of electrical railway work, it was the

successful formulating of the type of motor which persists even to the present day as a general standard, and his patents refer to the mechanical construction of this motor and its details. In the beginning of his induction motor work, he was concerned with methods for calculating its performance and with systems of operation which brought into its own the squirrel-cage motor. His patents of that date cover details of winding such as the chording of the coils, methods of getting variable speeds, and many mechanical details. At this period (1897), he wrote his classic paper on the polyphase motor, which is still authoritative. Later, when induction motors were used on railway locomotives and for the propulsion of ships, the patents cover systems of operation and distribution, questions of phase conversion and voltage regulation, schemes of winding and connecting to get various speeds, and all the broad phases of motor application. In this period come his patents on phase conversion and application of the Scott connection to phase converter winding, methods of phase balancing which have been most useful in the art. Toward the end of his life he was making theoretical analyses of the relations between different concepts of induction motor design and performance. Some of these studies have been published posthumously and would no doubt have resulted in other patents suggested by this work.

Similarly, in railway work the first single reduction railway motor was followed by work on direct-current railway generators and by rotary converters and systems of control, each with its own list of patents. The synchronous features of the rotary converter led to work on damper windings and work on equalizer connections,

which in a practical sense made possible modern direct-current and synchronous machine performance as it exists today. Another interesting outgrowth of his rotary converter work was the form of frequency-changer which bears his name, whose rotor was similar to a rotary converter armature and whose stator carried no windings whatever. Later he proposed other forms of frequency-changers, but it seems probable that he was responsible for the idea of the possibilities of a commutator used as a frequency-changing means. The patents of this period reflect this activity. A little later in railway work came the single-phase commutator motor development with its group of patents covering the principal features of a number of well-known electrifications. Then came the phase-converter locomotive, and later, the motor-generator locomotive, and the patent list shows that he left his impress on each.

Meanwhile, as a third major activity he was carrying on the development of turbo generators and many mechanical and electrical patents resulted in this field. The broad field of marine work yielded its patents, ranging from induction motors to gyroscopic stabilizers. All this work on apparatus inevitably yielded a wealth of ideas on systems of operation and distribution. There are thirty-one patents under this classification alone.

The complete list of patents gives a mental picture of a vista flanked by all the apparatus and all the electrical systems in common use for more than thirty years and establishes beyond question the fact that no summary of his accomplishments could be justly completed without adding to his name the title "inventor."

EDISON'S ELECTRIC LIGHT[1]

Rarely, if ever, in the history of invention, have the labors of any one man excited such keen and wide-spread interest as those of Thomas Alva Edison, the brilliant young American scientist, in his researches in electric lighting. For nearly sixteen months the entire civilized world has eagerly grasped every item of intelligence that leaked out from the inventor's laboratory, and watched earnestly for more. Opinions pro and con regarding the probabilities of his success have flooded the publications of Europe and America. On the one hand, the believers in his success pointed with pride to his past record, remembering how he had made a telephone out of a simple piece of chalk, involving the discovery of a new property of electricity; how he had succeeded in sending four messages simultaneously on one telegraph wire in opposite directions; how by his phonograph he had given tongue to a piece of tin-foil, causing it to speak like a human being; how he had performed scores of other miracles of science; and they blindly pinned their faith to his genius, believing him capable of doing all that he set out to do, be it ever so difficult of accomplishment. On the other hand, many scientific men doubted the practicability of his work, although such men as Tyndall and Sir William Thomson asserted that what he had undertaken was not a chimerical idea.

It seems incredible that Edison has succeeded in making his electric light out of a little piece of paper—paper of the same character and texture as that on which these words are written. It is asking, perhaps, as much of faith for credence as was necessary to cause men to believe that his phonograph was not, after all, ventriloquism. Yet incredible as it may seem, a little piece of paper which you might blow away with a breath gives out the electric light. It becomes no more affected, so far as destructibility is concerned, than platinum—one of the most infusible of metals—under the heat of a tallow candle. And from this piece of paper is obtained a pure and unadulterated light, a globe of sunshine, without deleterious gases, without noxious vapors, indifferent to wind or weather, requiring no matches to ignite, giving out no smoke or flame, possessing the uniformity and steadiness of the sun itself in clear weather, and withal a light cheaper in production than the cheapest oil.

Not the least curious in the contemplation of this wonderful achievement of science is its simplicity. The construction involves no incomprehensible intricacies. It is nature in nature's garb. A small piece of paper, a piece of cardboard, a cotton thread—for all have been used with almost equally good results—is subjected to an intense furnace heat, and the charred remains placed in a vacuum. An electric current is then sent through the same, and the electric light is given.

Before describing in detail the system, it may not be uninteresting to briefly review the history of electric lighting, and bring to mind some of the leading principles underlying electricity as a medium of illumination.

[1] *Harper's Weekly*, January 3, 1880. See page 21 and Mr. Scott's Foreword.

Among its other properties, electricity has that of developing heat. If a current of electricity is passed through a conductor offering considerable resistance to the passage of the current, such conductor becomes heated, the degree of heat being proportioned to the intensity of the current and the character of the conductor. The phenomenon is strikingly shown when the conductor used is platinum or carbon. A rod of either of the latter substances, when a sufficiently powerful electric current is used, may be heated to a white incandescence, emitting a beautiful light. This method of producing light from electricity is known as the incandescent method, and is the one adopted by Mr. Edison.

A second method, known as the voltaic arc system, is the passing of a powerful electrical current between the points of two pieces of carbon slightly separated from each other. By this means there is produced a light of an intensity estimated at almost one-half that derived from the sun on a clear day. This latter method of electrical illumination has been found eminently adapted for dock-yards, ships at sea, workshops, fortifications, parks, and other large spaces, and within the past few years its employment has been quite extensive both in Europe and this country.

In order to produce satisfactory electrical illumination by either the voltaic arc or incandescent system, it was found necessary at an early day to obtain electricity in some more suitable manner than by the galvanic batteries previously used. An opportune discovery by Ampère, developed by Oersted and Arago in 1820, and later by Faraday, to the effect that powerful electric currents can be produced by the rotation of magnets near each other, opened a wide field for experiments in electric lighting. Pixii, in 1832, was the first practically to apply the discovery. He constructed a machine consisting of a permanent magnet and an electro-magnet, with mechanism for revolving one directly in front of the other, which revolutions generated strong currents of electricity that could be carried off by wire, and made to give the voltaic arc. From this primitive machine such excellent results were obtained that before long ingenious inventors in different parts of the world began improving upon it, until there were finally developed machines called dynamo and magneto machines, which required for operation engines of several horse-power, and which gave out torrents of electricity at a comparatively small cost. As an illustration of the cost may be mentioned the case of Gilmore's Garden, in New York City, where all during the past summer fourteen machines of the Fuller Electrical Company supplied, under contract, an equal light to that previously obtained from gas, at exactly half the cost, the system used being the voltaic arc system.

But however suitable is the latter system for the illumination of large spaces, electrical difficulties, principal among which is the difficulty of subdividing the light, render it unsuitable for general domestic illumination. The latter being Mr. Edison's object, and being satisfied, after careful study of the subject, that the voltaic arc system presented insurmountable obstacles he turned his experiments all in the direction of the incandescent method.

Edison began his labors in electric lighting in the latter part of September,

1878. After many experiments upon different substances, he selected platinum as the one most suitable to be made incandescent. The first requirement obviously was one which would prevent the platinum from melting when the current became too powerful. His first invention was an ingenious contrivance for diverting the electric current from the platinum whenever the latter became heated to a point near its melting-point. How this was accomplished will appear from the following description, taken from the inventor's records:

"Electric lighting has been produced by a coil or strip of platinum or other metal that requires a high temperature to melt, the electric current rendering the same incandescent. In all such lights there is danger of the metal melting, and destroying the apparatus, and breaking the continuity of the circuit. My improvement is made for regulating the electric current automatically passing through such incandescent conductor, and preventing its temperature rising to the melting-point, thus producing a reliable electric light by rendering conducting substances incandescent by passing an electric current through them. In my apparatus the heat evolved is made to regulate the electric current so that the heat can not become too intense, because the current is lessened by the effect of the heat when certain temperatures are received, thereby preventing injury to the incandescent substance by keeping the heat at all times below the melting-point of the incandescent substance. Fig. 1 represents the electric light apparatus. The platinum wire *a* is a double spiral, with its two ends terminating at the posts *b c*, to which the conductors *d e* are connected. A circuit-closing lever, *f*, is introduced into the electric circuit, the points of contact being at *i*, and there is a platinum or similar wire, *k*, connected from the lever *f* to the head-piece, or other support, *l*. The current from a magneto-electric machine, a battery, or any other source of electric energy, is connected to the binding posts *n o*, and when contact at *i* is broken, the current passes from *o*, through the lever *f*, wire *k*, support *l*, wire *e*, post *c*, platinum coil *a*, post *b*, and wire *d*—a metallic connection to binding screw *n*. The wire *k*, being small, is acted upon by the electric current, and heated, and by its expansion the lever *f* is allowed to close upon *i*, and short circuit the current. The contact point *i* is movable, and adjusted so that the circuit will not be closed until the temperature of the apparatus arrives at the desired height, and by diverting a portion of the whole of the current, the temperature of the incandescent conductor is maintained in such a manner that there will be no risk of the apparatus being injured, or the conductor fused. The electric light may be surrounded by a glass tube, or any other suitable device, such as two concentric glass tubes, with the intervening space filled with alum-water, or other bad conductor of heat,

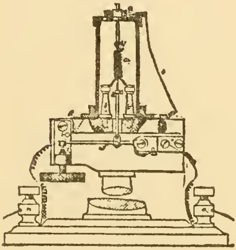


Fig. 1.—The First Lamp

the object being to retain the heat of the incandescent metal, and prevent loss by radiation, thus requiring less current to supply the loss by radiation."

Various other forms of lamps embodying the principle described were experimented upon, but only for the inventor to find that the repeated pressure of the expanding rod *k* upon the lever *f* had the tendency to bend the former, thus making frequent adjustment necessary. Other difficulties were also presented, and after a while the form was abandoned. It was soon afterward followed by a lamp the heat from which expanded a diaphragm, which diaphragm opened and closed a circuit, and cut off, when a certain temperature was reached, further electricity from the incandescent material. This plan worked quite well in practice; but it, like its predecessor, presented obstacles that rendered it unsuitable.

The next improvement was a device for obtaining more light-giving surface from the platinum. He wound the latter as a spiral, first having coated it with a non-conducting coating that was not injured by excessive heat. With this he employed a new regulating device, and introduced into the electric circuit a resistance coil that maintained a nearly uniform resistance in the electric circuit when the current was not passing through the lamp as when it was so passing, thereby making it so that other lamps in the same circuit were not affected by the extinguishment of one or more lamps.

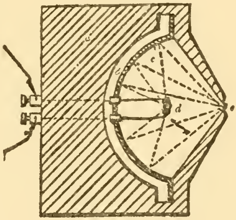


Fig. 2

The inventor next followed with a new regulator, and a meter for measuring the amount of electricity used.

A departure from the previous styles was the inventor's next move. He constructed a lamp by which the platinum was made to give the light only indirectly, its rays converging upon a piece of zircon, causing the latter to become luminous. Fig. 2 shows the same: *a* is a mass of non-conducting material; *b* is an air-space; *c* is a polished reflector of copper, with gold or coated platinum; *d* is a platinum, iridium, spiral, or other body, that becomes incandescent by the passage of electricity through the same; *e* is a thin piece of zircon or iridium that receives the heat rays thrown off from the reflector, and these heat rays are concentrated at *e*, and bring the metal or other substance at *e* to a vivid incandescence, producing a light more intense and brilliant than that produced by the spiral *d*. After numbers of modifications and improvements in this style of lamp, the inventor returned to his former method. Passing over many quaint and ingenious devices next in order for want of space, we find the inventor using for the light-giving substance powdered metallic oxides inclosed in small tubes of magnesia and lime. These gave good results, but the experimenter was far from satisfied.

From this Mr. Edison next turned his attention to carbon rods, in which he

made scores of experiments, only once more to return to platinum. Many more weeks of work on the latter demonstrated that platinum, as commonly used for incandescent electric lighting, and as he had been using it, was worthless. He found that the action of the electric current upon the platinum slowly but surely disrupted it, so that after a little use the microscope revealed myriads of cracks in the platinum in various directions, many of which extended nearly to the centre. He also found that under the action of the electric current the platinum lost weight. A further dispiriting discovery about this time was to the effect that the glass globes surrounding the electric light soon became coated with black deposits of platinum, thus obstructing the light. Thus had gone for naught many weary months of toil.

But the indefatigable scientist was equal to the occasion. He concluded to use the platinum under new conditions. To that end he adopted a style of lamp consisting of a glass globe, from which he extracted the air, the air-pump at the same time eliminating the air and gases from the platinum. By thus treating the platinum he obtained a metal eminently suited for incandescent lighting. Under the microscope a piece of platinum thus prepared was found to have a polish exceeding that of silver, and the cracks, before such a source of annoyance, were all gone. Not only this, but the troublesome deposit of platinum on the inside of the globes were done away with. The inventor had practically before him a new metal, and one that surpassed his expectations, for it gave out a light of twenty-five standard candles, while the best light previously attained was less than ten. Writing in this connection at the time, the inventor says: "I have further discovered that if an alloy of platinum and iridium, or if platinum alone, be coated with the oxide of magnesia, and subjected to the vacuum process described, a combination takes place between the metals and the oxides, giving the former remarkable properties. With a spiral having a radiating surface of three-sixteenths of an inch, light equal to that given by forty standard candles may be obtained, whereas the same spiral, not coated by my process, would melt before giving a light of four candles. The effect of the oxide of magnesia is to harden the wire to a surprising extent, and render it more refractory. A spiral made of this wire is elastic and springy when at dazzling incandescence. I have also found that chemically pure iron and nickel drawn in wires and subjected to the vacuum process may be made to give a light equalling that of platinum in the open air. Carbon sticks may be also freed from air in this manner, and be brought to a temperature where the carbon becomes pasty, and if then allowed to cool, it is very homogeneous and hard." At this stage of his experiments the inventor ascertained another truth, viz., that to practically subdivide the electric light the wires should be connected in the form known as "multiple arc," and the lamps should be made to have a resistance to the passage of the current equal to at least one hundred "ohms," or electrical units of measurement. "I find it necessary," he writes, "to reverse the previous practice of having lamps of but one or two ohms resistance, and construct lamps which, when giving out their light, shall have several hundred ohms resistance."

The lamp at this period was in quite a satisfactory condition, and the inventor rejoiced at the near completion of his labors. He had brought up platinum for incandescent lighting purposes to a degree of perfection it had never before reached; the subdivision of the light was a matter of accomplishment, the regulation of the electric current to prevent melting of the platinum was perfect, and the problem of economy had been demonstrated. A new discovery, however, was in store. Toying one night with a piece of lampblack mixed with tar (used in his telephone), he noticed, as he rolled it between his thumb and forefinger, that it readily became elongated, and the thought struck him that a spiral made of it might emit a good light under the action of the electric current. The experiment was soon tried, and while the result was satisfactory, it was not striking. Continuing in the same vein of reasoning, the inventor determined to try the carbonized remains of a thread in the same way. He thereupon placed a thread in an iron mould, and put it in the furnace. At the expiration of half an hour he took it out, and placing it in a glass globe, extracted the air, then turned on the current. The effect was surprising. The slender thread emitted a mild, beautiful light. The overjoyed inventor continued his experiments, trying a number of threads twisted together, straw, paper, card-board, and other substances. Paper and card-board gave the best results, the latter being a trifle preferable. The mode of preparation now adopted in the laboratory is as follows: By means of a small punch there is cut from a sheet of paper or card a strip of the same, in the form of a miniature horseshoe, about two inches from bend to end, and one-eighth of an inch broad. A number of these strips of paper are laid flatwise in a wrought-iron mould about the size of the hand, and separated from each other by tissue-paper. A light weight, consisting of a piece of retort carbon, is then laid on top to keep them from curling when subjected to heat. The mould is then covered, and placed in an oven, where it is raised gradually to a temperature of about six hundred degrees. This allows the volatile portions of the paper to pass away, and at the same time the mould retains the paper in its proper shape, and the paper is prevented from curling up or becoming distorted,

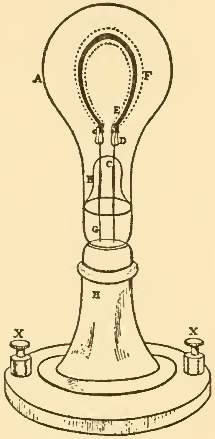


Fig. 3.—The Perfected Lamp

A. The Vacuum Globe. B. Interior Glass Crest, through which Wires pass to Light. C. Platinum Wires. D. Platinum Clamp. E. Carbonized Card-board. F. Dotted Line, showing Size of Incandescence equal to Sixteen Candles. G. Copper Wire to Metre and Generator. H. Wooden Stand. XX. Binding Posts.

as would likely be the case were the heat applied suddenly, or the light weight dispensed with. The mould is now placed in a furnace, and heated almost to a white heat, and then removed and allowed to cool gradually. On opening the mould, the charred remains of the paper—in other words, the carbonized filaments—are found. They are carefully lifted out with pincers placed in the lamp and secured to the platinum wires C (see cut). The air is then exhausted from the glass globe, and when a vacuum of about $\frac{1}{300,000}$ of an atmosphere is obtained, the glass is sealed, and the lamp is taken off the vacuum pump, and is ready to be placed in the electrical circuit for illumination. When the electric current is turned on, the carbon filament instantly becomes luminous, giving out a bright, beautiful, and pleasing light.

It is not alone the cheapness of the lamp as thus constructed that renders the discovery so valuable for electric lighting. The carbon filament seems to combine all the elements necessary for the purpose to which it is applied. The uniform resistance (each filament gives a resistance of about two hundred ohms), the infusibility and indestructibility, and other properties, the absence of any one of which would be fatal, are all concentrated in the little piece of charred paper. "It is as though the Almighty had decreed it," observed Mr. Edison, reverentially, in speaking of the peculiar properties found in the paper. For several weeks now the lamps have been burning in the inventor's laboratory, and every day's use only confirms his faith, and the faith of all who have seen them, that verily the problem of the electric light has been solved.

But the inventor's labors in search of the proper lamp did not constitute all the work required in perfecting a system of electric lighting. There were generators for the production of the electricity to be made, metres for measuring the amount consumed to be devised, methods of running the wires, proper degrees of relative conductivity, dynamometers, and a dozen other minor details going to make up the system. Experiments in all these branches were carried on simultaneously with those relating to the light proper, and some of them were as stoutly resisted by nature as was perfection in the lamp. Without entering into details, it will probably be sufficient to generally describe the generator and metre.

In the northern end of Mr. Edison's machine shop, at Menlo Park, is located the engine which furnishes the horse-power to drive the dynamo machines, and magnets which supply the current. The power expended is weighed by means of a device which consists of a line of belting driven by the main shaft, and running over a series of wheels, from the lower of which depends a box containing about a thousand pounds' weight. An ordinary platform scale registers the weight, and indicates the horse-power.

The generators constructed by Mr. Edison produce a greater percentage of current per horse-power than any similar machine yet known. They consist of two upright cylindrical electro-magnets, four feet in height and six inches in diameter, wound with large covered wires, and resting on a soft-iron basis, with concave faces forming its poles. The two blocks upon which the cores rest, as well as the bearings of the armature, are supported by a cross-shaped

brass casting. Between the poles horizontally is a cylindrical armature of wood wound parallel to its axis with fine iron wire, which is again covered with a series of wrapped wires, which terminate in insulated brass strips, which compose the outer surface of the commutator, and from which during the rapid revolution of the armature the current is collected by a brush-like series of short copper wires arranged accurately on opposite sides of the commutator, a complete drawing of which is shown on page 4.[1] A. Wire-covered spool. B. Brass terminals of covered wire. C, C. Copper ends for collection of current. D. Vulcanized fibre for insulation. E. Brass base. F. Oiler. G. Binding posts.

The current passes from the generators to a metre, which is also shown on page 4,[1] and the consumption is measured by means of the decrease in weight of a thin strip of copper suspended in the decomposing jar. The metre is simple, and is furnished with a safety-clutch, which is operated automatically, and designed to protect the lamps and wires, in event of any damage occurring by current or otherwise. Description: A, A. Binding Posts. B. Bar, or lever. C. Magnets. D. Safety-clutch. E, E, E. Resistance spools. F. Decomposing jar. G. Platinum safety-wire. H. Copper strip. X, X. Binding posts.

Another form of the generator, called the Faradic motor, is to be used for furnishing power to do light work, such as run a sewing-machine or pump water. The amount of electricity required to run it is only that required to furnish one light, and it may be used with or without the light. If, for instance, the housewife desires to sew, she has only to connect a small belt from the motor to her sewing-machine, and turn on the electricity. The cost will be the same as if she turned on a single electrical burner.

The motor is made in different sizes, depending on the amount of power required from it. The size suitable for running sewing-machines is two feet long, and weighs eighty pounds. It can be placed directly under the sewing-machine.

Mr. Edison's arrangements for the exhibition of his light are now complete. Wires run from the generators to the lamp-posts which line the road to the depôt, and also to a number of the houses and buildings. The fixtures in Mr. Edison's house are quite artistic, and are supplied with two and three lights each. Mr. Edison's home near the railroad has been brilliantly illuminated each night for several weeks, and the office, laboratory, and workshop, situated on the hill beyond, have not been in darkness.

MAGIC SQUARES

In Chapter IX under hobbies Mr. Lamme speaks of the recreation he had from different sorts of puzzles and of his interest in making magic squares. Such magic squares are described in his U. S. Patent #728,249, the drawing from which and a portion of the description are reproduced herewith.

[1] See illustration page 21.

No. 728,249. PATENTED MAY 19, 1903.

B. G. LAMME.

PUZZLE.

APPLICATION FILED JUNE 28, 1901.

NO MODEL.

Fig. 1.

19	48	5	58	3	64	21	42
45	18	59	8	61	2	43	24
60	7	46	17	44	23	62	1
6	57	20	47	22	41	4	63
51	16	37	26	35	32	53	10
13	50	27	40	29	34	11	56
28	39	14	49	12	55	30	33
38	25	52	15	54	9	36	31

Fig. 2.

142	1	44	103	46	97	140	7	94	49	92	55
4	143	102	41	100	47	6	137	52	95	54	89
101	42	3	144	5	138	99	48	53	90	51	96
43	104	141	2	139	8	45	98	91	56	93	50
110	33	12	135	14	129	108	39	62	81	60	87
36	111	134	9	132	15	38	105	84	63	86	57
133	10	35	112	37	106	131	16	85	58	83	64
11	136	109	34	107	40	13	130	59	88	61	82
126	17	28	119	30	113	124	23	78	65	76	71
20	127	118	25	116	31	22	121	68	79	70	73
117	26	19	128	21	122	115	32	69	74	67	80
27	120	125	18	123	24	29	114	75	72	77	60

WITNESSES

C. L. Belcher

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ATTORNEY.

In Fig. 1 of the accompanying drawings I have illustrated in plan view one embodiment of my invention, and in Fig. 2 a similar view of a modification.

The elements of my invention are blocks which severally have four unlike numbers on at least one face and which may be of any material and any thickness and surface dimensions, the blocks being all of the same form and dimensions, and therefore susceptible of many relative arrangements to form an invariable geometrical figure, but being susceptible of only one arrangement which constitutes a solution of the game or puzzle. In Fig. 1 I have shown sixteen blocks, each of which is divided by two lines perpendicular to each other into four equal squares, the whole number of small squares being therefore sixty-four, each of which is provided with a different number, the numbers being from "1" to "64," inclusive, and none being used more than once. The numbers on each block are such that their sum equals one hundred and thirty, and they are so arranged that there is a difference of one between two of the diagonally-disposed numbers and a difference of three between the other two diagonally-disposed numbers on each block. The entire number of blocks when arranged in the form of a square may be designated as the "primary" square, and this is composed of four secondary squares, each of which contains four blocks. When thus arranged in a complete square, there are eight horizontal and eight vertical lines of eight numbers each and two full diagonals of eight numbers each. The partial diagonals on each side of the main diagonals obviously vary from seven numbers to one, and each two diagonals on opposite sides of the main or full diagonals, which together comprise eight numbers, may be designated as "complementary" diagonals. The problem for solution with this combination of blocks having the face numbers thereon, as above described, is to so arrange the sixteen blocks as to present certain peculiar arithmetical combinations and results as follows: When the blocks are properly arranged, the sum of each four numbers in a horizontal line on any two adjacent blocks is one hundred and thirty, and consequently the sum of the numbers in each horizontal line across the primary square amounts to two hundred and sixty. The sum of the four numbers in each vertical line on any two adjacent blocks is one hundred and thirty and therefore, the sum of the numbers in each vertical line of the primary square is two hundred and sixty. The sum of the four numbers on each block is one hundred and thirty and also the sum of each four numbers constituting a square, whether these numbers be on one, two, or four different blocks, is one hundred and thirty. For example:

$$19+48+45+18=130$$
$$48+5+18+59=130$$
$$45+18+60+7=130$$
$$18+59+7+46=130$$

The sum of the numbers in each of the main diagonals of the primary square is two hundred and sixty and the sum of the numbers in each two complementary diagonals is also two hundred and sixty. Considering now the secondary

squares which severally comprise four blocks, we find that the sum of the numbers in each of the two main diagonals is one hundred and thirty and the sum of the four numbers in any two conplementary diagonals is one hundred and thirty. These conditions are true with reference to each of the four secondary squares. The sum of the alternate numbers in each full diagonal and in each two complementary diagonals of each secondary square is sixty-five—*i.e.*, one-half the sum of all the numbers in each full diagonal, each two complementary diagonals, each four numbers constituting a square, each vertical line of four numbers, and each horizontal line of four numbers.

In Fig. 2 I have shown a primary square composed of thirty-six blocks, each of which is divided into four squares, the same as the blocks shown in Fig. 1, the entire set of numbers on the primary square being from one to one hundred and forty-four, inclusive. The combination and arrangement of numbers are the same as in the square shown in Fig. 1, and already described, except that the sum of the numbers on each block is two hundred and ninety and the sum of each four numbers forming a square, whether on the same block, on two blocks, or on four blocks, is two hundred and ninety. Also the sum of each horizontal line of numbers on any two adjacent blocks is two hundred and ninety and the sum of each vertical line of four numbers on any two adjacent blocks is two hundred and ninety. In this form there are nine secondary squares of four blocks each, and the sum of the numbers in each main diagonal, as well as in each two complementary diagonals in each of these secondary squares, is two hundred and ninety. The sum of the numbers in each diagonal of the primary square and in each two complementary diagonals of the primary square is eight hundred and seventy—*i.e.*, three times the corresponding sums on the secondary squares. In this embodiment of my invention the sum of alternate numbers in each full diagonal and in each two complementary diagonals of each secondary square is one hundred and forty-five—*i.e.*, one-half the sum of each combination of numbers above described as pertaining to the secondary squares.

In each embodiment of my invention the difference between two of the diagonally-disposed numbers on each block is equal to the lowest number of the entire set, and the difference between the other two diagonals on each block is equal to three times the lowest number of the entire set.

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INDEX

INDEX

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